DONATI'S COMET, OCTOBER 4, 1858.

MEMORIALS

OF

WILLIAM CRANCH BOND

Director of the Harvard College Observatory 1840–1859

AND OF HIS SON

GEORGE PHILLIPS BOND

Director of the Harvard College Observatory 1859–1865

BY

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INTRODUCTORY NOTE

MY earliest recollections are connected with visits to the Harvard College Observatory, where Professor George Bond (who had married my cousin) was assistant astronomer, and subsequently Director. As a lad I had a boundless admiration for the science to which he gave his life, as well as a deep gratitude for the unfailing kindness with which he and his father received my young friends and myself on our rather frequent pilgrimages to the observatory, and for the glimpses which they gave us of inspiring things. As years went on, I myself became a student of his chosen science, and went over the ground — theoretical or practical — which he had trod, and learned to admire his remarkable qualities of mind, and to appreciate his very considerable and original contributions to astronomy. It has also been my fortune, as an observer, to repeat some parts of his work, and to obtain, in this way, an even more accurate judgment of his persistent thoroughness.

No adequate biography of either of the Bonds has been written, and the lack is especially noticeable in the case of the son. A number of documents that would serve to illustrate their lives are not now to be found; all that are available have been collected and copied by his daughters. At their request, I have undertaken to put these materials into an orderly form. The difficulties of the task have not been small. No one who was not at the same time an intimate and a contemporary could overcome them entirely.

The present volume will have a value as a contribution towards the early history of astronomy in America. The development of the science has been so amazing that we forget that the Harvard College Observatory was founded as late as 1840; that it was one of the very first of college observatories; and that its first Directors — the Bonds, father and son — had it in charge until 1865, only a generation ago. Nearly every American observatory has been founded since 1840. American science is scarcely more than half a century old. The day will soon come — it is now here — when we shall look back with wonder and gratitude to ask who were the men who laid the wide and deep foundations which already maintain so noble an edifice.

In the little group of able men who were the fathers and founders of American astronomy, the Bonds have their secure place. It is the object of this volume to set forth their lives and work, and to point out their many and varied services to their country and to science.

E. S. H.

The Lick Observatory,
January, 1897.

ACKNOWLEDGMENTS

Those who have been concerned with the preparation of this volume beg to return their grateful thanks to the many friends who have aided, in one way or another, in the collection of materials.

William Cranch Bond.
1789-1859.

CHAPTER I

WILLIAM CRANCH BOND 1789-1859

AMONG the papers left by WILLIAM CRANCH BOND is a memorandum relating to the original home of the family in Cornwall and to their migration to America. The following paragraphs are extracted from Mr. BOND's manuscript:—

"My father, WILLIAM BOND,"* he says, "in the year 1780 found that he was nearly related, if not heir-at-law, to the WILLIAM BOND who held the estate of Brendon, in the parish of St. Dominic, County of Cornwall, England, A. D. 1711.† From this stock branch off the BONDS of St. Austell, Cornwall. My father visited Brendon in 1780, and saw the dates of entry of all the possessors of that estate, from the time of WILLIAM the Conqueror down to the date of his visit, beginning with the inscription on an oaken tablet in the hall—'WILLIAM BOND came to Brendon, A. D. 1066.' This was followed by a succession of WILLIAM BONDS, without any other name intervening, down to the year 1780. In 1833 I went there with Brother HUGH CURTIS. It was then held by a WILLIAM BOND,

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* Born in Plymouth in 1754.

† This estate has been held by a WILLIAM BOND for fully eight centuries.

whose brother JOHN confirmed to me orally the foregoing account. I was received very kindly, and saw the date 1066 on or over a stone arch. It is a very pleasant place, situated in a rich agricultural district. . . . Within the church were several monuments; in one corner were two marble tombs of knights; the effigies, carved in marble and cross-legged, indicating thereby that the warriors had been to the Holy Land. My grandfather's brother, JOHN BOND, owned two estates, Trevarner and Trevanion, near the town of St. Austell, and he there resides.

"My great-grandfather was the first of the family who went to live in Plymouth from Cornwall. My great-grandfather died there at a very advanced age. My grandfather, THOS. BOND, was established in Plymouth as a chemist [and surgeon] in 1750. He was much respected by his fellow-townsmen and was eminent in his profession. Many anecdotes illustrative of his grave, resolute character have been handed down to us.

"THOMAS BOND married THOMAZINE PHILLIPS. He died in Plymouth in 1782. His widow died in 1803. Of his four children (sons), only one, WILLIAM, left issue.

"After a previous visit to this country in 1784, my father [WILLIAM, son of THOMAS] settled in Falmouth, Casco Bay [now Portland], in May, 1786, bringing with him his wife and two children, THOMAZINE and THOMAS. The brig *John* in which they came had been chartered by my father for that and commercial purposes. He was made a free citizen of the Commonwealth of Massachusetts by special act of the General Court, passed November 23, 1785."

The wife of WILLIAM BOND was HANNAH CRANCH.

The home of the CRANCH family was in and around Kingsbridge, Devonshire. They were all much given to books, with a marked artistic bent. Speaking of the family, one writer says: "The race of CRANCH all had literary, artistic, and poetic tendencies. There were always authors, poets, musicians, and travelers in the family."

JOHN CRANCH, born in 1744, was an artist. Among other things, he painted a picture of the death of CHATTERTON. He was also a lawyer and an author. He lived in England, but was made a Fellow of the American Academy of Arts and Sciences of Boston.

The children of WILLIAM BOND and HANNAH CRANCH were:—

THOMAZINE ELIZABETH FIELDER;
THOMAS, died in infancy;
WILLIAM, died in infancy;
THOMAS, came to America an infant;
HANNAH CRANCH, born in America;
WILLIAM CRANCH, born in America.

RICHARD CRANCH, the uncle of Mrs. WILLIAM BOND, settled in Massachusetts in 1746, and became Judge of the Court of Common Pleas. He was a highly respected and influential citizen of the Colony. Harvard College conferred upon him the degree of M. A. in 1780.

From a sister of RICHARD CRANCH are descended the PEABODYS, ELIZABETH, MARY (Mrs. HORACE

MANN), SOPHIA (wife of NATHANIEL HAWTHORNE).

Urged by the relatives of his wife, WILLIAM BOND determined to emigrate to America. He arrived in Boston in May of 1786, and settled in Falmouth (now Portland), Maine, where he engaged in shipping lumber to Bristol, England; and he also established a branch station on Frenchman's Bay. This venture proved unsuccessful, and in 1790 he and his wife and children returned to Boston, where he began business as a silversmith and watch and clockmaker, trades he had learned in London in his youth. In 1793 he established the firm still called by his name, and began to import clocks and watches from England. It took time to build up a business, and his family had a long, hard struggle with poverty. His wife, HANNAH CRANCH, was a woman of great force of will, a stern sense of justice and integrity, and marked vigor of intellect; an uncompromising Puritan housewife, strict in the performance of every duty, and equally strict in exacting of others their dues. Her stern standard of domestic discipline is still remembered by the oldest surviving granddaughter. In her presence the children of the family never spoke except in subdued whispers; and when she entered a room all rose and remained standing until she was seated. These were the manners of the age, but she is said to have been peculiarly strict in enforcing them, and even her grown children stood in awe of her.

William Bond was a true Cornishman—fiery, impulsive, generous, affectionate and indulgent to his children, but subject to violent outbursts of passion. He had also a true Cornishman's taste for mining, and lost considerable sums of money in mining ventures in Maine and at Gay Head, Massachusetts. He and his wife never ceased to regret their immigration to America. A loyal love of England was characteristic of the family for many years. In household customs, manners, and traditions, they were thoroughly English down to very recent days.

William Bond lived to a great age, dying at his son's house in Cambridge, in 1848, in his ninety-fifth year. He is still remembered as a quaint figure, wearing his hair in a queue in the fashion of a bygone time, with rosy cheeks and bright, sharp eyes; brisk and vehement, even after his mind began to fail. In his extreme age he became a Roman Catholic, through the influence of his personal friend, Bishop Cheverus. At his death he left two daughters and two sons—Thomas and William Cranch Bond.

William Cranch Bond was born in Portland, Maine, September 9, 1789. During a visit to England

(July 18, 1819) he married for his first wife his cousin, SELINA CRANCH, in Kingsbridge, Devonshire. She was the mother of his six children—WILLIAM CRANCH BOND Jr., JOSEPH CRANCH, GEORGE PHILLIPS, RICHARD FIFIELD, ELIZABETH LIDSTONE, SELINA CRANCH—and died in 1831. She is remembered as a beautiful woman, with soft and gentle voice and manners. After her death BOND married her elder sister, MARY ROOPE CRANCH, who left no children. His second wife was a woman of strong, determined character, capable, and very ambitious. The household expenses were willingly cut down to save the money needed for the purchase of costly books and instruments. She was ready to make any personal sacrifice for the science to which her husband was devoted.

BOND loved science for itself, and cultivated it with a kind of passion, never thinking, in his early days, of even communicating his results. He had been observing the great comet of 1811 for months before his observations came to the knowledge of Professor FARRAR of Harvard and Dr. NATHANIEL BOWDITCH of Boston.

It was necessary for young BOND to do his part towards supporting the family. He left the public school at an early age and soon became an admirable workman. His attention was turned to astronomy by the remarkable total solar eclipse of 1806, when the sun was hidden for no less than five minutes. At the age of fifteen (1804) he had constructed a satisfactory shop chronometer, and about the same time a quadrant, which was also a very serviceable instrument. His first sea-going chronometer was made in 1812, and the severe test of a voyage to India and return proved it to be an excellent timekeeper. These early days are recalled by a paragraph from a letter written after his death by his elder brother, THOMAS BOND, to his son.

EXTRACT FROM A LETTER OF THOMAS BOND TO GEORGE BOND.

Feb. 21, 1859.

. . . The scenes of our youth come back to my remembrance like a pleasant dream. Your dear father and I were for years almost inseparable friends and companions. . . . He was the mildest and best-tempered boy I ever knew, and his very remarkable mechanical genius showed itself very early. He was the best hand for the manufacture of box traps, snares for partridges, quails, and rabbits when not more than twelve years old. He was not very strong physically, but constant exercise established his health. He had few advantages of education; but a powerful mind and retentive memory enabled him to press forward. We were poor, and he had to help in the support of the family, but his courage and perseverance carried him through all. "We ne'er shall look upon his like again."

In an old French book he found the description of the chronometer used by the celebrated navigator, La Perouse, and determined to try his skill in making one on the same plan, to be kept in motion by weights instead of springs. Notwithstanding his constant attention in the shop, he carried out his plan, and it still remains as a memento of his skill and patience when but fifteen years old.

I suppose it would incline the Astronomer Royal to laugh, could he see the first transit instrument used by us at Dorchester,—a strip of brass nailed to the east end of the Champney house, with a hole in it to see a fixed star and note its transit,—this in 1813. When we moved into the Hawes house, he procured a good granite block. We dug a deep hole, and placed it at the west end of the house, and got Mr. Alger to cast a stand for the transit instrument,—a small one,—which I think belonged to Harvard College. From this time he began to live among the stars, and now I trust he wears the starry crown. He was faithful here below, my best friend on earth, and I trust to meet him again before long.

. . . .

Yours ever, Thomas Bond.

After his father's death, George Bond wrote a long memorandum to the Hon. Edward Everett (sometime President of Harvard University), rehearsing the circumstances of his career. This letter is printed in what immediately follows, with a few omissions, and with the addition of a few notes of explanation.

MEMORANDA RELATING TO THE LIFE AND SCIENTIFIC LABORS OF WILLIAM CRANCH BOND, LATE DIRECTOR OF THE OBSERVATORY OF HARVARD COLLEGE, COMMUNICATED TO HON. EDWARD EVERETT, MARCH, 1859, BY GEORGE PHILLIPS BOND.

Several years since, at the suggestion of some of the members of his family, my father collected various memoranda, relating chiefly to the early part of his life, and subsequently combined them in an autobiographical sketch.

Knowing that this manuscript had been prepared by his own hand, and that it was not long since in his possession, we have been greatly disappointed at not finding it among his papers, although no pains have been spared in searching for it. Its loss is the more to be regretted, because it had been relied on as an authentic source of information in regard to many incidents, which it will now be impossible to recover, as by trusting to it too confidently we came to neglect other memoranda. This circumstance has occasioned delay and trouble in collecting even the imperfect information given below. I have dwelt somewhat minutely on the incidents of my father's early life, for it was then, in a stern conflict with obstacles which few would have had the resolution to meet, that he displayed the earnestness and strength of his native predilections for astronomy. . . .

My grandfather [WILLIAM BOND], a native of Plymouth, England, claimed to be the representative of an ancient family residing at Brendon Manor, St. Dominick, Cornwall. On a visit to the place in 1833, his son found in the old manor-house, and in the monuments of the parish church, sufficient confirmation of the traditionary claim. The estate had unquestionably been held by a WILLIAM BOND from a remote period. Among the monuments in the church were two tombs of Crusaders, traditional WILLIAM BONDS, Knights of Brendon. The tenure of the estate has been always under the condition, that it should be held by one of that [Christian and sur-] name, and my grandfather at one time took steps to obtain possession, failing, it would seem, only from his own neglect to press the claim. The crest and motto of the family were sufficiently aspiring and aptly astronomical. A demi-Pegasus (one of the constellations) on a field of golden stars,* with the motto, "*Non sufficit Orbis.*"†

The discouraging circumstances of my father's early life are alluded to in my letter to Mr. CURTIS. I have always understood that his situation up to manhood, and even for years after, was one of peculiar trial and hardship. It was at this period of life, usually so full of animation and buoyancy, that he speaks of himself as "nearly heartbroken and in despair of ever being able to accomplish anything." The expression bespeaks the sensitiveness of his disposition, and a dejection unnatural in one so young. His mother, HANNAH CRANCH, as was fit, was ever the *confidante* of his plans, and the consoler of his distress. She was a woman of a well-cultivated mind and high excellence of character; one who could sympathize in his high aspirations, though she could not relieve the pressure of adversity.

It would not be proper to enter into the details of those hardships with which he was at this time encompassed. He rarely alluded to them, except in general terms. One of the companions of his youth has said emphatically, "They talk of early disadvantages; *he* went over *red-hot ploughshares,*" and all who then knew him intimately use the strongest

* Not *on* a field, but *semée d'étoiles d'or.*

† The arms are: *Argent*, on a chevron *sable*, three besants.

expressions with reference to the stern severity of his lot. In his boyhood a modest reserve and a quick sensitiveness were as prominent as in later life; yet there was a resolute spirit beneath this veil, or he would never have risen superior to frowning fortune. This simplicity of manner and shrinking from ostentatious display did not wholly conceal from his playmates a consciousness of superior capacity; he could be silenced easily, but rarely diverted from his purpose. A design once formed in his mind seemed to become a part of his very being, and was pursued with an unfaltering aim. To this invincible perseverance he owed everything. It is not for us to condemn his persistence, sometimes beyond the bounds of reason, in his original convictions. Whatever he accomplished was done in a quiet, unobtrusive way; but if opposed, a determined, persevering energy was manifested, equal to any emergency, and seldom to be disappointed of its end. These are said to have been traits of his boyhood — they certainly characterized his after life. He early evinced the ingenuity and fertility in mechanical contrivances for which he was subsequently distinguished. At the age of ten [1799] he made a wooden clock, and became famous among his playfellows for his skill in the manufacture of traps, toys, etc., even extending his ambitious efforts to an imitation of philosophical apparatus, with which, when a mere boy, he illustrated to his companions the experiments of a public lecturer in Boston. [A ship's chronometer made by him at the age of twenty-three years] is now in our possession, and bears the date 1812. The construction of such an instrument requires a high degree of skill, which he must have attained with scarcely any aid from instruction, as the original profession of his father, to whom he was apprenticed, was that of a silversmith. There could have been no facilities at that time in the country for acquiring knowledge of the art. Some hints on the subject he found in an old French book, containing a description of a timekeeper used by the celebrated navigator LA PEROUSE. He was ambitious that it should be of home manufacture, and for that reason substituted a weight for the maintaining power, in room of the mainspring commonly used. The latter he could not make, and would not procure from Europe. I have before me a journal of the performance of this chronometer on a voyage from Boston to Sumatra, in the ship *Cyrus*, commanded by Captain THOS. B. CURTISS. It indicated the longitude of the ship as accurately as the majority of the chronometers now made would have done.

His first *astronomical* apparatus was a sundial and pieces of string held at arm's length, with which he plotted the stars and comets, after the fashion of FERGUSON. These were succeeded by other contrivances better adapted to the purpose. It is a fact not without interest that for many years preceding the war of 1812, the period of our greatest commercial prosperity, the "rates" and "errors" of nearly all the chronometers employed in the foreign trade of Boston were derived from instruments made by his hand.

The history of his [independent] discovery of the comet of 1811* shows him at that time as an attentive observer of the heavens. He had previously, for want of a telescope, "been in the practice of going to a deep well, and, shading his eyes from stray light, would direct his eyes toward the bottom for some minutes," and with this preparation faint objects among the stars were more easily distinguished. Instead of attempting to acquire reputation from the discovery, he was so careless

* It was discovered in Europe, March 25, 1811,—by BOND, independently, April 21.

on this point that it took months for the intelligence to travel four miles to Cambridge. On the other hand, he applied himself most industriously to collect observations with such apparatus as he could command. To watch the motions, and record the positions of the heavenly bodies, was an occupation perfectly congenial to his tastes, which evidently brought with it its own reward. It was his constant practice, from the time when he first came into possession of appropriate instrumental means, to record astronomical phenomena, often with no other apparent motive than a love of the occupation. For thirty years this was done, not merely without compensation, but to his manifest pecuniary disadvantage. This consideration, it is probable, never entered his mind.

In this period we find him zealously tracing the courses of comets, collecting observations of lunar culminations, occultations, and eclipses of the sun, determining by different methods the position of his observatory and connecting it by trigonometric surveys with neighboring points, and in other ways evincing the strength of the ruling passion by the sacrifices which were made to gratify it. Nor was his attention confined to astronomy; the kindred sciences of meteorology and magnetism were not neglected. Even on his journeys it was his custom to take with him a sextant and artificial horizon and a chronometer to find the latitudes and the longitudes of the places visited.

The circumstances connected with his visit to Europe in 1815, must have exercised a favorable influence upon his astronomical training.* Apart

* At a meeting of the President and Fellows of Harvard College, held May 10, 1815, it was *voted* that the President, Treasurer, and Mr. LOWELL, with Professor FARRAR and Dr. NATHANIEL BOWDITCH, be a Committee to consider the subject of an Observatory, and report to the Corporation their opinion upon the most eligible plan for the same, and the site. BOND was at that time going abroad (aged twenty-six years),

from the invaluable opportunity it afforded of inspecting some of the best observatories of the world, and of witnessing the processes of instrument-making in various branches, the confidence thus reposed in him must have been highly gratifying and encouraging. The trust was discharged to the entire satisfaction of the college. The memoranda made on this occasion have been preserved, and probably furnish the most exact account extant of the observatories visited, as regards their plan and manner of mounting the instruments. A part of his traveling expenses was paid by the college after his return. He probably studiously concealed from those who sent him on this commission the fact that he was left in England in a state of utter destitution. . . .

The longitude of his observatory in Dorchester, adopted just thirty years since, agrees precisely with the latest determination of the position of the observatory of Harvard College, allowing for the difference of meridians. The latitude also presents as exact an accordance as could be attained with the instruments in his possession, confirming his remark: "I was satisfied that no repetitions with the instruments would have given me greater confidence in the result."

In the first house which he owned [in Dorchester] the only parlor was sacrificed to science, and forthwith converted into an observatory. A huge granite block, some tons in weight, rose in the center of the room, and the ceiling was intersected by

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and was given the mission of making examinations of the building and instruments at Greenwich, and of consulting with instrument makers. His letter of instructions (dated June 23d) from Professor FARRAR and his account of his mission are printed in the first volume of the *Annals* of the Harvard College Observatory. Plans of the Observatories of Greenwich, Edinburgh, etc., were obtained, and advice was received from POND (Astronomer Royal), GROOMBRIDGE, BENTLEY, Sir WILLIAM HERSCHEL, EDWARD TROUGHTON, and others.

a meridian opening. My recollection will just carry me thirty years back to this room and its mysterious paraphernalia. I can recall, too, in the garden and neighboring fields the stone blocks for the support of instruments, meridian marks, etc. Like the men of old, wherever he sojourned a stone was set up as a memorial. His antipathy to an insecure foundation many would have thought extravagant; the tremor of an instrument would annoy and fret him as a harsh discord does the cultivated ear of the musician. . . .

Every year, as his means allowed, some addition was made to the resources for observation; but adversity still waited on him, and he was obliged, as a constant practice, after the whole day had been devoted to business, to spend hours at his work-bench. He made it, in fact, a rule of life to earn enough by his nightly labor at his profession as a watchmaker to meet the current household expenses. That so much industry and application should have failed in placing him in a position of competence will not surprise any one acquainted with his methods of conducting business transactions, for which, as far as his own pecuniary advantage was concerned, he had no capacity. The making of a good bargain was to him the most incomprehensible of problems.

Between 1825 and 1830 he was engaged in collecting materials for an investigation on the comparative rates of marine chronometers at sea and on shore, and effectually disposed of the question, as far as the interests of navigation are concerned, in a communication to the *American Academy*, containing in a few words the substance of a thorough discussion of the subject. No mention is made in the memoir of other experiments conducted by him on the influence of changes of temperature, of the presence of large surfaces of iron, etc., on the performance of the same instrument. It deserves notice, that although his conclusions on these investigations were at variance with the opinions of men high in authority and influence, they are now known to be correct. Variation of temperature is now recognized as the grand source of disturbance, once vaguely attributed to the influence of "traveling," or of a "sea voyage."

On receiving in 1838 an appointment from the United States Government to coöperate with the exploring expedition under the command of Lieut. Com. CHARLES WILKES, although the equipment of his observatory was amply sufficient for the objects specified in his instructions, he would content himself with nothing short of new buildings and a new suite of instruments. For the first time in his life —he was now verging on fifty—the object of long-restrained aspirations seemed within his reach. We may censure the manifest imprudence of committing himself to a plan of operations which would have tasked the energies of three men; but we must admire the generous enthusiasm evinced in the undertaking. In a short time a new observatory of ample size was erected [in Dorchester] and provided with complete apparatus, including all that was requisite for an investigation of the magnetic and meteorological elements.

The magnetic apartment was a long building, built entirely of wood, no iron nor metallic substance of any description entering into its construction. Thus provided, he assumed the truly Herculean task of recording on term days* the position of the magnetic needle at intervals of *ten seconds* (instead of the *five minutes* proposed by the committee of the Royal Society) throughout the twenty-four hours, enlisting the aid of every friend

* Certain stated days.

THE DANA HOUSE. First Observatory of Harvard College — 1840.

whom he could induce to volunteer to watch the mysterious excursions of the magnet. . . .

The circumstances connected with my father's removal to Cambridge, in December, 1839, are detailed in the remarks made at a recent meeting of the American Academy by one who had been through life his constant friend, the Hon. JOSIAH QUINCY.

["Ex-President QUINCY, upon the obituary occasion referred to, made this interesting statement as to the initiation of his project for Mr. BOND's removal to Cambridge: 'This proposal, so in unison with his pursuits and talents, I expected would be received with pleasure. But it was far otherwise. In the spirit of that innate modesty which predominated in his character, and apparently cast a shadow over all his excellent qualities and attainments, Mr. BOND hesitated, doubted his qualifications for the position. He said his habits were not adapted to public station; that our combined apparatus would be small, and that something great might be expected; that he preferred independence in obscurity to responsibility in an elevated position. He raised many other objections, which need not here be repeated, as they were overcome.'

"At the date of this interview the President found Mr. BOND well established in a profitable manufacturing business, happily situated in his domestic and neighborhood surroundings, with an avocation fascinating enough to occupy all his leisure, and a fame extensive enough to satisfy his own modest estimate of his abilities. There was no pecuniary betterment for Mr. BOND in the suggested change. Mr. QUINCY could only offer him what he had already, a family domicile;* so that the proposal might warrant an adaptation of SIDNEY

* No salary was attached to the office until 1846.

SMITH's famous phrase, and be described as an invitation to come to Cambridge and 'cultivate astronomy upon a little oatmeal.' In so phrasing it there is no disparagement of the college; it was the day of small things, of pennies, not dollars, in the college treasury. But the event speaks the praises of Mr. QUINCY, whose sagacity was unfailing, and before whose persuasiveness and energy difficulties in administration were wont to give way, and of Mr. BOND, whose unselfishness and loyalty to science were proof against pecuniary considerations. In mental traits each was in many respects the complement of the other, and it is not too much to say that these two were pre-eminently the founders and builders of the observatory."*]

. . . The following comments will furnish some additional particulars, which may be of service. Prior to the occupation of the new observatory, magnetic and meteorological observations engrossed a large share of attention. The Magnetic Observatory had been thoroughly equipped with the aid of funds contributed by the American Academy,† and by private subscription. The system proposed by the Royal Society was followed up to the end of March, 1843, with a considerable extension, effected with the assistance of Professors LOVERING and PEIRCE and of the "Harvard Meteorological Society," composed of students in the university. This association, suggested by Professor PEIRCE, continued in activity for more than a year. My father's labors were not, however, at this time wholly diverted from astronomy, as is witnessed by the addition of above two hundred lunar culminations to his previous collection, beside records of occultations and eclipses, meteors, auroras, etc. In

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* History of the Harvard College Observatory, by DANIEL W. BAKER, page 13.

† $1000 given in 1840.

November, 1841, by the untimely death of his oldest son, WILLIAM CRANCH BOND Jr., he was deprived of a devoted assistant. Though cut off at the early age of twenty, my brother had become an accomplished and zealous observer. He possessed an unusual taste for mathematics, and gave high promise of future eminence. His natural bias toward astronomy was far stronger than mine.* While I was in a manner pressed into the service, he entered of free choice. My father's loss by this affliction I have always felt to be scarcely greater than my own. His disposition had much of his father's gentleness; and to have enjoyed the intimate sympathy in scientific pursuits of such a companion in after life would have been to me an inestimable advantage.

You will find in all historical notices of the new observatory allusions to the influence upon the public mind of the sudden apparition of the great comet of 1843, and I think this has not been overrated. Professor LOOMIS, in his sketch of the principal observatories of the United States,† ascribes the general movement which took place in this country in 1835–38 in favor of an extension of our means of celestial observation, to the return of the comet of HALLEY in 1835–36. Thus, projects for observatories were set on foot at Williams College in 1836; Hudson, Ohio, 1836–37; Philadelphia, 1837–38; West Point, 1837–38, and the National Observatory at Washington had the first effective impulse given to it in 1838. My father's appointment by the government as Astronomical Observer occurred in 1838. Georgetown and Cincinnati [observatories] followed soon after, beside private enterprises of a similar nature.

*See the next chapter, however.

†Recent Progress of Astronomy in the United States, 1856, 8vo.

No country in the world has at this moment a larger number of the finest instruments, and even of observatories, as far as stone piers, bricks and mortar, and a place in college catalogues will give a claim to the name. We should probably be short of the truth if we estimate the present number of these institutions, provided with means for furnishing astronomical data of a high order, at fifty. *Three* only manifest any vitality. Is it not clear, from the neglect and disease of the rest, that men are rare who will make great personal sacrifices for science? What would my father not have given for the early opportunities now so freely afforded?

It is not without interest to me to connect decisive epochs in his career with notable events in astronomy. The total eclipse of 1806 was witnessed under such a rare combination of circumstances that centuries may elapse before one of equal grandeur occurs. Its duration at Boston was "*five minutes*, almost the greatest possible." The impressiveness of the scene can scarcely have been surpassed in any other natural phenomenon. An eyewitness speaks of "the perfect day without a cloud, the slowly-increasing gloom, the falling dews, the darkening west, the trembling stars looking out upon the scene, the birds and animals seeking the protection of man, the silence and *tears* of the spectators. Then the arrowy rays shot out, rekindling nature." The great comet of 1811 confirmed the already deep impression made on his mind by this event. HALLEY's comet, as we have seen, became a turning point in the scientific progress of the country; that of 1843 heralded the successful movement at Cambridge in that year. The last object which my father viewed through the great telescope was the comet of DONATI, on the 5th of October, 1858. On what more fitting scene could that eye have rested! It was near sunset, the sky beautifully clear and tranquil. By a singular coincidence, it was the day on which the comet had attained its greatest splendor, the day, too, on which it passed its conjunction with the brilliant star *Arcturus*, an occasion memorable in astronomical history.

Between 1843 and 1844, in the erection of the new observatory and in the preparation for the mounting of the instruments, he derived much advantage from previous experience. Of this I will mention one or two illustrations. To secure stability in the position of the piers upon which the instruments immediately rest, he insisted that no expensive attempts should be made to fit or smooth the faces of blocks resting one on the other, but that *three* bearings alone should be admitted at the base of each pier, situated horizontally as far from the center of gravity as practicable. These three points necessarily fixed the position of the plane of the base as effectually as if the surfaces were fitted with all possible care and expense. By attending to this condition, not only is the desired end perfectly accomplished, but much is gained in the readiness with which the preliminary adjustments can be made. An instance occurred in the mounting of the great equatorial. It was necessary to set the massive block of granite, on which it rests, in position upon the capstone before the instrument arrived from Europe. Of course it could not be accurately adjusted until the instrument had been mounted, and the errors ascertained from celestial observation. When this had been done, it was found that the block with the instrument upon it, weighing together nearly fifteen tons, must be moved with the utmost nicety to bring the latter into the meridian. By a record in our notebooks, I find that this was effected in between *five* and *ten* minutes, with the aid of a few wedges and a jackscrew. Had the surfaces been cemented or accurately fitted, a good deal of delay and trouble would have been unavoidable. The plan is so simple that one might suppose it could scarcely have been overlooked, but instances to the contrary, attended with no small inconvenience and expense, are known to have occurred.*

The present plan for the revolving dome—the mounting upon spherical balls instead of wheels or rollers—has the same character of simplicity. [It was proposed by my father in 1815 or 1816.] . . . Contrary to my father's advice, indeed against strenuous opposition on his part, the dome was in the first instance mounted upon a system of rollers, which entirely failed of accomplishing the object intended. His own views of what was required in the building of the observatory differed so widely from those of the architect as to occasion a good deal of disquietude, and he would never admit his responsibility for its external appearance, nor for its internal arrangements, excepting in the piers, the plans for the dome, and the machinery of meridian openings, observing chair, etc.

One of his first cares at the new observatory was to determine, in conjunction with Colonel GRAHAM of the Topographical Engineers, the latitude by the method of prime vertical transits. The particulars are given in the third volume of the *Memoirs of the American Academy*. This has been pronounced by high authority to have been at the time "the only latitude in the country determined with the requisite precision for the higher problems of astronomy."

In this year, December, 1844, a small equatorial telescope [two and three-quarter inches in aperture] was temporarily mounted in a building to the west

* This is a reference to the construction and installation of the piers of the Dudley Observatory.

Harvard College Observatory—1847.

of the great pier. This served a useful purpose by supplying extra-meridional observations on comets, etc., until the arrival of the great refractor. Among these were the solar eclipses of May, 1845, and April, 1846,—the transit of *Mercury* over the sun on May 8, 1846, etc. With the assistance of this instrument, the planet *Neptune* was seen for the first time in America on the 21st of October, 1846, the evening on which we first received intelligence of its discovery at Berlin. The observations subsequently made were incorporated in the well-known investigations of Prof. SEARS C. WALKER. Upon the fine comet of June, 1845, we obtained exact measurements on three dates (the 2d, 4th, and 6th of June), sufficient therefore to determine its elements, before a single reliable determination of its place had been made in Europe. The very remarkable phenomenon of the division of the comet of BIELA presented itself in this small telescope as an elongation of the parent body on the 14th of January, 1846, one day later than its recognition by the astronomers at Washington, where its double character was first detected [with the 9.6 inch equatorial], but one day in advance of European observers.

A series of drawings of solar spots was executed in 1847–48–49 by my father, also with this telescope. They number about two hundred and fifty sheets, often containing several groups on each.

On reference to the following passages in the "*Annals*, Vol. I, Part I," viz. p. lxxx, "Mr. BOND has for five years," etc.; p. lxxxvii, "It being understood," etc.; p. xciii, "And when it is known," etc.,—you will perceive that his services to the college, up to August, 1846, had been rendered without pecuniary compensation.*

*** His contract with the United States Government terminated in August, 1842. How well he had acquitted himself in this engagement will appear from the liberal proposals made**

The great refractor was received on the 11th of June, 1847. The mounting was commenced on the 23d and completed on the 24th of the same month without the slightest accident.

Among the earliest objects critically examined were the nebulæ of *Orion* and *Andromeda*, disclosing new features in each. The very complicated character of that first named renders its correct delineation a most difficult undertaking. My father's drawing has been pronounced by Sir JOHN HERSCHEL, not an incompetent judge, the best that has appeared. About two years since, M. OTTO STRUVE criticised the positions of the stars included in the engraving somewhat freely, or rather to my mind, offensively; and his disparaging remarks have been republished and circulated in the newspapers in the country, probably through the agency of one of the "sacred brotherhood of science." However this may be, I have given the subject a most thorough revision in the winters of 1857–58–59,* and am now engaged upon the reductions. This seems due to a proper regard for the reputation of the observatory, in the only instance of an unfriendly disposition manifested toward it by an European astronomer.

The planet *Saturn* was subjected to a thorough scrutiny, the fruits of which are mentioned on p. xix of the "*Annals*, Vol. I, Part I," and more at length in two or three papers in the *Memoirs of the American Academy*, and in Vol. I, Part II, of the "*Annals*."† In this connection I may mention

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to him in 1845 by the Secretary of the Navy and the Chief of the Hydrographical Bureau to take charge of the National Observatory.

***The Memoir is printed in Volume V of the *Annals* of the Harvard College Observatory.**

†The reference is to the discovery of the eighth satellite of *Saturn*—viz. *Hyperion*—on September 19, 1848. The discovery was made by G. P. BOND, September 17th and 18th, and verified by W. C. BOND, September 19th; and by W. LASSELL, in England, on the same night.

that some of the best authorities have made conflicting and erroneous statements in relation to the phenomena and constitution of the rings of *Saturn*. I am convinced, for instance, that the eminent English writer, Mr. GRANT, is altogether in error in the explanation of the bright protuberances upon the edge of the ring "at its disappearance." These phenomena are discussed in an appendix to our volume of *Saturn* observations. He assigns also to LAPLACE the discovery of the fluidity of the rings, to which you will find a satisfactory reply on p. xix of our "*Annals*."

Our numerous observations and drawings of *Jupiter*, in 1848–49, have not been published, and the same may be said of the valuable collection of drawings of solar spots made by my father in 1847–48–49, above referred to.* During the same interval we formed, with much pains, a chart of the stars in the cluster of *Hercules*. I am sorry to add, that this also is condemned to the same obscurity with the results of many other laborious undertakings which we are without the means of publishing. A notice of the cluster will be found on p. cxxxvii of the "*Annals*."

The mass of the planet *Neptune* has been deduced both by Professor PEIRCE and myself from the measurements of the satellites, made with the aid of the great refractor, and has been found by Professor PEIRCE to represent in a most satisfactory manner the observed irregularities in the motions of *Uranus*.

With the history of the discovery of *Hyperion* and of the new ring of *Saturn* you are already familiar. Observations for the parallax of the sun have been made by three different processes. In

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***The drawings of sun spots were published in Vol. VII of the *Annals* of the Harvard College Observatory, by the care of Professor WINLOCK, Director.**

1849–50 by simultaneous observations at Cambridge and Santiago, Chili, upon the planet *Mars* in opposition, and in 1857 upon *Venus* at its inferior conjunction.

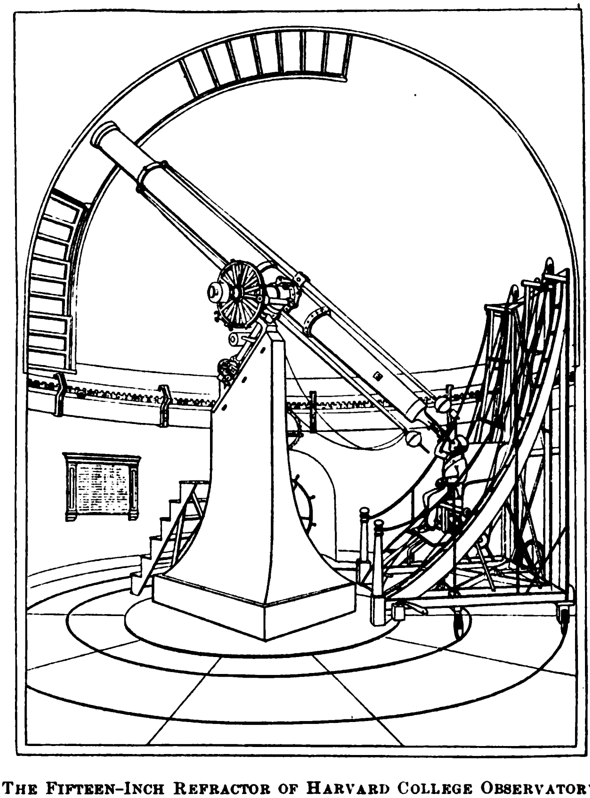
The parallax was also determined in 1849–50 by a method probably for the first time put in practice; namely, by the displacement of *Mars* in right ascension at considerable hour angles east and west of the meridian. Of this process, which has the peculiar advantage that it does not require the coöperation of a second observatory, Mr. AIRY, in a lecture before the Astronomical Society, expressed the opinion that it was the best of all known methods. For his statement of its advantages, see *Monthly Notices of the Royal Astronomical Society*, 1856–57, p. 219. He was not probably at the time aware that it had been used in America several years before.

The zone observations (of which one volume has appeared, Vol. I, Part II, of the *Annals*, and a second is ready for press) were set on foot in 1852. It has always been our design to continue them as rapidly as the means of publication will allow, as a part of the current work of the institution.

Passing over matters of less importance, I will allude only to the following additional operations in which the observatory has been intimately concerned during the period of my father's directorship.

The observatory has contributed much to the public welfare through its constant connection, by correspondence, advice, the communication of observations, and in various other ways, with nearly all of the great surveys which have been carried on on this continent during the last twenty years.

Our northern and southern boundary lines, the surveys of our eastern and western coasts and of our inland seas, the proposed routes of the Pacific railroads, and many similar enterprises in the



The Fifteen-Inch Refractor of Harvard College Observatory, and the Observer's Chair.

United States, in Canada, the British provinces, and Mexico, have been more or less indebted to Cambridge for some measure of the success with which they have been conducted.

Cambridge is now the central geographical point of this continent. The charts of the British Admiralty, and the most remote scientific station on this hemisphere, Santiago, in Chili, recognize this fact. This distinction, conferred in the first instance in deference to the authority of the data accumulated by my father's industry at Dorchester, has since received the most ample confirmation. Our longitude has undoubtedly been investigated with more care than that of any other spot on the globe.* By the employment of the telegraph, all the more important localities accessible by that means are readily referred to the primary station.

One other honorable distinction to which the observatory may justly lay claim is its association with the earliest origin and improvement of the two methods recently proposed for the registration of celestial phenomena, viz. the use of the electric current in the registration of the transits of stars, and the application of photography to astronomy. My own opinion of the influence which photography is destined to exert upon the future of astronomy is presented in an article upon "Celestial Photography," published in the *American Almanac* for 1859. It is now more than ten years since the electro-magnetic process came into use. It has, until quite recently, met with anything but favor abroad (England excepted). Continental

*In addition to the results from occultations, eclipses, and moon culminations (eight hundred of the latter, and two or three hundred of the former have been observed at Cambridge), three special chronometric expeditions between Boston and Liverpool have been conducted by my father, at the expense of the United States Coast Survey, for the purpose of furnishing an independent determination of longitude.

astronomers, who have been at no pains to conceal their indifference to its claims, are now reduced to the necessity of making the fullest admission of its superior excellence in every particular. No alternative is indeed presented but to sacrifice either their prejudices or the interests of science and their own reputation as accurate observers. There will doubtless be a few of the present generation who will choose the latter course, but they cannot stay the manifest destiny of the electric method to take rank with the telescope and the clock among the indispensable requisites for the solution of the future problems of astronomy.

In the first volume of the *Annals* of the Harvard College Observatory there is a list of the principal contributions to the institution since its foundation. In 1839 thirty gentlemen, well-known merchants of Boston, gave $100 each to the observatory funds, and the American Academy gave from the Rumford fund $1000. The last sum was expended in furnishing the magnetic apparatus. In 1843 there are contributions from ninety-five societies and individuals, amounting in all to $25,730. Mr. David Sears gave $5500 for the construction of the main tower, the American Academy gave $3000, the Society for the Diffusion of Useful Knowledge gave $1000, and the contributions of individuals make up the rest. The large equatorial was purchased and the observatory constructed from this fund. In 1846 there are again contributions from thirty-three gentlemen of Boston, amounting to $5170, which was used in completing the equipment. In 1847 Mr. David Sears gives $5000. In 1848 an endowment of $100,000 was made by the will of Edward Bromfield Phillips, a friend of George Bond.

Edward Bromfield Phillips, a young man of large fortune, was a classmate of George Bond in Harvard. He was a modest, shy lad, whose life at home was not happy, and who had few friends. He was much attached to Bond and interested in his pursuits, though he was not himself a specially brilliant scholar. After his graduation an unhappy love affair led to differences with those with whom he was then living. His brief unhappy life came to a sudden and a tragic end. He died by his own hand. In his will he left a legacy of $100,000 to the observatory, and the Phillips Professorship of Astronomy in Harvard University commemorates his name. It was understood that his gift was prompted by his friendship for the younger Bond and by his interest in his work.* The gift made a distinct improvement in the affairs and standing of the observatory. William Cranch Bond became the first Phillips Professor, and the office has been held by all the directors of the observatory successively, namely, George Bond,

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* See *Annals* Harvard College Observatory, Vol. I, Appendix, p. cxlii.

JOSEPH WINLOCK, EDWARD C. PICKERING. It is now held by the chief assistant in the observatory, ARTHUR SEARLE, and is thus for the first time separated from the directorship.

In the year 1851 there are again contributions of small sums from sixty gentlemen of Boston, which amount in all to $4575. It is interesting to note that the various subscriptions come from the same families year by year.

In the year 1855 ex-President QUINCY made an endowment of $10,000, the interest on which was to be devoted solely to the publication of the *Annals* of the observatory. The endowment was made in memory of President QUINCY'S father, JOSIAH QUINCY, who died in April, 1775, leaving a name inseparably connected with the history of the American Revolution.

During the years 1839–55 the gifts to Harvard College Observatory from its friends (mostly gentlemen of Boston and vicinity) amounted to $154,475. The direct appropriations from the general funds of the college were but small. The salary of the director was $1800, and of his son $600, at first. The support given to the Harvard College Observatory in its early days by the generous and intelligent community which surrounds it is continued to this day, when it has an income nearly twelve times that of a generation ago.

The elder BOND was the soul of kindly hospitality, but his nature was shy and retiring, and his whole thoughts given up to his profession. There seems, however, to have been constant entertainment of scientific colleagues and of old family friends. For these "open house" was always kept, and the hospitality was generous, kindly and dignified. The children of the family were one and all impressed into the all-absorbing service of astronomy, and saw comparatively little of the young social life around them. They were in close touch with the earnest work of their father; his friends were theirs, rather than the youth of their own age; and being like him, quiet, modest and serious, they made comparatively few friendships. But these, once made, were peculiarly tender and enduring.

In his later years WILLIAM BOND was very frail in health, and subject to distressing seizures of heart trouble. The slightest excitement or emotion put his life in extreme peril. He is remembered in his last years as a gentle, kindly old man, serene and placid, with silvery hair and a complexion as rosy and delicate as a little child's. His temperament was deeply religious; his piety genuine and unaffected.

His sons, WILLIAM, JOSEPH, GEORGE and RICHARD were all, in their various ways, men of marked ability. I believe that they all died under forty years of age. WILLIAM was rarely gifted, and by his early death, when in his senior year in Harvard, America lost a brilliant man of science. From his childhood he was devoted to the theoretical and practical study of astronomy, and became his father's zealous and very proficient assistant. From the time of the removal of the family to Cambridge, he "voluntarily devoted himself to the interests of the observatory from its first establishment, by daily and nightly labors in observation and computation." It is to be noted that there was no pecuniary compensation for any of this work. No salaries were paid until 1846, seven years after BOND's removal to Cambridge.

JOSEPH and RICHARD were both able men. The latter, especially, was very ingenious, and was the ever-ready and intelligent coadjutor of his father and brother in the horological work of the observatory. He had a large share in contriving the "spring-governor," and devised a new clock-escapement,* which received a medal at the Paris Exposition of 1867. The spring-governor (chronograph) was similarly rewarded at the Crystal Palace Exposition of 1851.

RICHARD BOND took the liveliest interest in the

* Which is described, with a plate, in BRUENNOW's *Astronomical Notices*, No. 21, October 11, 1860.

affairs of the observatory, and was the first to see the secondary tail of the great comet of 1858.

During the elder BOND's incumbency the observatory was a popular resort for all classes of citizens. Crowds used to visit it. It was the pride of many Boston men of business who had contributed toward its establishment, and its novelty was a source of great attraction. It is to be regretted that the healthy interest in astronomy thus created could not have been fostered; but the throngs of daily visitors interfered so seriously in the systematic work of the institution that it became necessary to refuse admission to any sightseers not provided with letters to the director.*

The first volume of the Harvard College Observatory *Annals* (1856) contains a quantity of data relating to the early history of the institution and the *Reports* of the director, etc., to 1855. Much of this history has been told in other words in the present chapter. A few extracts from the *Annals* may be collected in this place to supplement what has gone before.

It is interesting to remark that the vessel which took Professor WINTHROP and his party to Newfoundland to observe the transit of *Venus*, in 1761, was fitted out at the public expense; and that again, in 1780—in the midst of the war—the Government

* See a letter dated February 28, 1859, in Chapter IV.

directed the Board of War to fit out a galley to convey a party from Harvard to observe the eclipse of the sun at Penobscot (Maine). In 1839 the Dana House property was bought by the college for observatory purposes, and in 1844–45 the latitude was deduced by Professor PEIRCE from observations by the BONDS and by Colonel GRAHAM of the army. The site for the new observatory was purchased in 1841, the buildings were constructed by 1846, and the great telescope was mounted June 24, 1847.

Just what the exact relations of the Perkins Professor of Astronomy (Professor PEIRCE) and of the Astronomical Observer of Harvard College (Mr. W. C. BOND) were originally intended to be it is difficult to say. In 1847 the Board of Visitors say: ". . . These instruments are necessary to complete the apparatus indispensable to facilitate the concurrent labors of the Perkins Professor of Astronomy and of the observer." In 1845 JOHN QUINCY ADAMS had written to the President of the college (JOSIAH QUINCY) that the line of division between the duties of the observer and those of the Perkins Professor should be accurately drawn. Correspondence on this subject exists, but it is not readily available, nor (now) very important. In 1849 the Visitors "deem it their duty, unanimously, to state that, in their judgment, the occupation, care and custody of the house and buildings, and of all the instruments of the observatory, should be vested in the director of the observatory for the time being, and that on him should rest, exclusively, the responsibility for their use, by himself, or by any person whomsoever; and that, also, the right in the observations made by him shall vest in him exclusively," etc.—subject, of course, to the control of the corporation. At one time the observatory was attached to the Lawrence Scientific School, but it was finally organized as "a separate department of the university," with an "independent existence" and a "separate faculty of its own," in 1855 or thereabouts.

The official name of the institution was fixed in 1849 as "The Observatory of Harvard College." In their report of 1851 the Visitors strongly recommended that it should be in the future known as "The Observatory of Harvard University." For some reason this recommendation was not adopted, and the old name still serves to-day. These small signs point to some administrative friction, which is of small import now, but which had its importance at the time.

The relations of the Perkins Professor to the Bonds were very friendly until the year 1851, when George Bond printed his paper on the constitution of the rings of *Saturn* shortly before Professor

PEIRCE published another memoir on the same subject. So far as can now be known from the printed papers and manuscripts, the priority rests, and should rest, with the younger BOND. The two investigators approached the subject from different sides; each made a step forward in showing that the rings of *Saturn* could not possibly be continuous solids; and both announced the (erroneous) conclusion that they were fluid. The relations of Professor PEIRCE with GEORGE BOND (not with his father) became somewhat strained, and he gave something of his support to covert reflections against the observatory, which began to appear about this time. Directly after the death of the elder BOND he went to the observatory to announce to his son that he was himself a candidate for the vacant directorship. He was not elected to it, and became an open enemy. With all his great qualities, Professor PEIRCE was hot-tempered and hasty, and could not be quite just and fair in his estimate of BOND's work. In particular, the fine memoir on the comet of DONATI excited his angry criticism. In Chapter V of this book I have tried to give an account of the immense services of PEIRCE to American science. It is much to be regretted that it was not possible for the little group of astronomers in Cambridge to work in entire harmony. Each one of the group had qualities of genius or talent not possessed by the others, and their coöperation would have produced results of even higher value than those attained separately — for at the end they were broken up into as many separate groups as there were individuals.

The following letter may find a place here. It is not possible to understand the circumstances of the lives of the BONDS without some knowledge of their personal relations with men of their own profession. Nothing more will be given in this book than what seems to be necessary and just — just not only to the BONDS but to all concerned. Grievances now nearly half a century old may be allowed to slumber after the briefest allusion to them.

LETTER FROM WILLIAM BOND TO HON. WILLIAM MITCHELL.

HARVARD COLLEGE OBSERVATORY,
CAMBRIDGE, 1858, September 22d.

My Dear Friend:—

I thank you for your kind and encouraging letter and for the copies of your report on the observatory. This printing of our reports, I am inclined to think, will be of advantage to the observatory, more particularly if they should enter into details of what has been accomplished in its appropriate pursuits. The distribution of such reports would often tend to arrest prejudicial influences arising from a misunderstanding of the exact nature of our occupation, and misrepresentations, such, for example, as may be found in Dr. B. A. GOULD's report to Lieutenant GILLISS on the observations made during the United States Astronomical Expedition to Chili for the purpose of determining the amount of *solar parallax,* and to which we contributed. Doctor GOULD . . . speaks in a very disparaging manner of our observations. In reference to the Cambridge observations he says, page lxxi, "Observations of right-ascension are utterly without avail for our purpose"; and again, at page clvi, with the same reference, he says: "Right-ascension observations not only have no relation to the problem before us, but are utterly incapable, under any circumstances, of furnishing a trustworthy value for parallax." Previous to penning these remarks Doctor GOULD had been informed of the series of differential observations which had been made at this observatory on the planet *Mars, east* and *west* of the meridian, near the opposition in 1849–50, which have since been reduced and have given consistent and satisfactory results [for the parallax], and proved that this method is the best, all things considered, that has ever been practiced. Of this we feel perfectly assured, and in the last number of the Royal Astronomical Society's *Notices,* Mr. AIRY [the Astronomer Royal] strongly recommends this method to the attention of astronomers as the best hitherto devised, and has had a list of stars proper for observing with *Mars* prepared for the next opposition of 1860, which will be just ten years after us. . . . Again, in his report to the Superintendent of the Coast Survey, speaking of our spring-governor [chronograph], he says, that "since Mr. BOND *has introduced the German principle of regulation,* the machine works perfectly." Now the fact is, there is not, and never has been, anything introduced into the spring-governor bearing the least analogy to the German principle of regulation of rotatory motion. The same course has been pursued by Doctor GOULD in regard to our zone observations and the photography of stars.

Now if our reports were made out a little more in detail, printed and circulated, I think these misrepresentations would hardly be ventured upon.

The credit for the invention of a thoroughly satisfactory chronograph was assigned to Mitchel, and not to Bond as it should have been, at various scientific meetings, by those who had every reason to know better; and the credit for laying the foundations of astronomical photography is given to others than the Bonds, as it should not have been, in a biography published so late as 1895 (Biographical Memoirs of the National Academy of Sciences, Vol. III, page 439, 1895). It would seem that the pursuit of abstract scientific truth should foster magnanimity, a love of justice, a moral elevation—but apparently, it does not always do so. Astronomers may take some comfort, however, in the fact that the poets are no better! Every one recollects Victor Hugo's early review of Alfred de Vigny's *Eloa*, where the poem is praised to the skies; and Hugo's later revision of the review, after their quarrel, where the name of John Milton is substituted for that of De Vigny, and where *Paradise Lost* takes the place of *Eloa*, but everything else remains unchanged!

Astronomers will be interested in little hints with regard to the equipment and work of various European observatories which are to be found throughout the first volume of the *Annals*, especially in a memorandum by the Astronomer Royal, on p. xcvii; but they need only a reference here. The discovery of a new satellite to *Saturn*, of its dusky ring, the chronometric expeditions between Europe and America in the years 1849–55, are spoken of elsewhere. In 1852 the longitude of Halifax was determined, at the expense of the Coast Survey, under the direction of WILLIAM BOND. And in the same year the chronographic method was introduced at the Royal Observatory, Greenwich. BOND's chronograph had received a gold medal at the Crystal Palace Exposition of 1851.

The scientific work of WILLIAM CRANCH BOND is spoken of with some detail in Chapter V. In estimating his place among scientific men it is necessary to take into account the times in which he lived and the circumstances which surrounded him. He was born in the first year of the French Revolution; he was absolutely self-taught; practically no astronomical work was done in America before 1838. When Admiral WILKES was seeking for coadjutors to prosecute observations in America, during the absence of his exploring expedition, he was indeed fortunate in finding two such assistants as BOND and GILLISS. Their assiduity was beyond praise, and it led each of them to important duties. Bond became the founder and Director of the Observatory of Harvard College, while Gilliss is the father of the United States Naval Observatory at Washington, as well as of that of Santiago de Chile, the oldest observatory in South America.

Cambridge, though the seat of the most ancient university in America, was but a village in 1839. The college could offer no salary to Bond, but only the distinction of a title,* and the occupancy of the "Dana House," in which his first observatory was established. The last observation in Dorchester was made on December 25, 1839. The first at Cambridge is dated December 31. His work there, as elsewhere, was well and faithfully done, and it led the college authorities to employ him as the astronomer for the splendid college observatory, which was opened for work in 1847. At that time the two largest equatorials in the world were those of the Imperial Russian Observatory (Pulkowa), and its companion at Cambridge. Each of these instruments has had a long and honorable history. Their work has been very different. Who shall say that one has surpassed the other?

The first American to be elected one of the fifty Foreign Associates of the Royal Astronomical Society (founded in 1820) was William Cranch

* Astronomical Observer to the University.

BOND, who was chosen in 1849. Following is a complete list of those elected from the United States:—

1849.	WILLIAM C. BOND.	1872.	LEWIS M. RUTHERFURD.
1850.	BENJAMIN PEIRCE.	——	CHARLES A. YOUNG.
——	ALEX. D. BACHE.	1876.	GEORGE W. HILL.
——	O. M. MITCHEL.	1879.	ASAPH HALL.
——	SEARS C. WALKER.	——	C. H. F. PETERS.
1855.	F. F. E. BRUENNOW.	1881.	EDWARD C. PICKERING.
——	MATTHEW F. MAURY.	1883.	SAMUEL P. LANGLEY.
——	BENJAMIN A. GOULD.	1884.	EDWARD S. HOLDEN.
1863.	GEORGE P. BOND.	1889.	SETH C. CHANDLER.
1866.	TRUMAN H. SAFFORD.	1890.	LEWIS BOSS.
1872.	SIMON NEWCOMB.	1892.	WILLIAM L. ELKIN.
——	HUBERT A. NEWTON.	1894.	ALBERT A. MICHELSON.

Since the year 1823 the Royal Astronomical Society has given a gold medal for services to science. The first American to receive this medal was GEORGE PHILLIPS BOND. The medal has been awarded to the following Americans:—

1865.	GEORGE P. BOND.	1887.	GEORGE W. HILL.
1874.	SIMON NEWCOMB.	1894.	SHERBURNE W. BURNHAM.
1879.	ASAPH HALL.	1896.	SETH C. CHANDLER.
1883.	BENJAMIN A. GOULD.	1897.	EDWARD E. BARNARD.
1886.	EDWARD C. PICKERING.		

BOND received the honorary degree of M. A. from Harvard College in 1842, and was a member of the American Academy of Arts and Sciences in Boston, of the American Philosophical Society in Philadelphia, etc., and was counted among the corresponding members of the Institute of France, of the Accademia dei Lincei in Rome, etc., and was a Foreign Associate of the Royal Astronomical Society of London, as we have seen. The following extract from the *Proceedings* of the American Academy of February 8, 1859, gives the resolutions adopted after his death. They were moved by Professor PEIRCE.

Professor PEIRCE announced to the Academy the decease of the late WILLIAM CRANCH BOND:—

It is my sad duty to . . . draw the attention of the Academy to the loss of another of our most eminent associates, whose far-reaching and well-earned reputation has been reflected back from the older shore of the Atlantic in one of the distinguished honors so rarely conferred upon those of American birth. WILLIAM CRANCH BOND, the Director of the Observatory of Harvard College and Phillips Professor of Practical Astronomy, has ascended to the nearer study of the stars, and joined the constellation of the devout astronomers of past ages. . . . During seventeen years I have been Mr. BOND's colleague in Harvard College, and this interval comprises the whole period in which he had any favorable opportunity of astronomical observation. But his love for the science had been shown long before he came to Harvard, and even a quarter of a century earlier he made a careful survey of the Greenwich astronomy, at the request of Professor FARRAR, with direct reference to the superintendence of the erection of an observatory at Cambridge. This was in the year 1815, at a time when only a small fraction of the present members of this Academy had reached the age of manhood. . . . When Mr. BOND returned from England he set up a small observatory of his own, where he undertook the observation of occultations and eclipses. It was here that he developed one of the finest elements of genuine enthusiasm and true genius, that of accomplishing much with small means.

. . . While Mr. Bond was devoting himself to astronomy with simple and unassuming zeal, he attracted the kind and approving regards of men whose approbation and friendship were worthy of being secured, and who never deserted him. When in the year 1842 he was drawn to Cambridge by the strong hand of President Quincy; when the cause of the observatory was undertaken by the unflinching and irresistible vigor of my friend, Mr. J. Ingersoll Bowditch; when even the heavens came to our assistance, and that wonderful comet of 1843, appearing at midday in close proximity to the sun, and seeming to send off in a few hours its immense train of two hundred millions of miles in length, excited most opportunely a universal interest in celestial phenomena—it was then apparent that the affection for Mr. Bond was the chief strength of the occasion, and to that were we mainly indebted for the successful attempt to obtain the unrivaled equatorial of the university and to lay the foundations of the observatory. In the history of American science there is no more memorable epoch. . . .

The astronomical researches of Mr. Bond while at the observatory are so recent that I need only allude to them. By the habits of his life his attention was especially drawn toward the improvement of the instrumental means of observation. Hence we have from him, and under his administration—first, the ingenious observing-chair of the great equatorial; second, the spring-governor . . . ; third, the application of photography to the sun, moon, and stars.

In his original investigations he naturally restrained himself to those forms of observation which were fully within the reach of his own resources. He did not, therefore, seek those inquiries which could only be accomplished by long, intricate, and profound mathematical computations, but preferred those which were merely dependent upon the thorough discipline of the senses. He consequently availed himself less of the remarkable capacity of his instrument for delicate and refined measurements than of its exquisite optical qualities. But when observations were required which must be passed over to the computer, his skill was not wanting to the occasion. Thus, in conjunction with Major GRAHAM, he made that choice series of observations from which the latitude of the observatory was determined. His observations, and those made under his administration, upon the nebulæ of *Orion* and *Andromeda;* the interesting discoveries as to their revolution and peculiar configuration; the researches into the physical aspects of the different planets, and especially those upon the Saturnian system; and the remarkable discoveries of the inner ring and of the fluid constitution of the rings, and of the eighth satellite, need only be named. They are known to all; they have passed into the text-books of astronomy, and our children's children will be familiar with the name of BOND.

Permit me, sir, to embody my high estimation of Mr. BOND in the following resolutions:—

Resolved, That as fellows of the American Academy of Arts and Sciences we are grateful for the long and valuable services of WILLIAM CRANCH BOND, who has proved that an American mechanic can accomplish one of the highest positions in science, and whose astronomical discoveries have illustrated his country and his observatory, and stamped his own name honorably and indelibly upon the records of history.

Resolved, That in the simplicity and sincerity of his Christian life, which, purifying his spiritual atmosphere from all influences which might disturb observation, imparted that serenity and tranquility which charmed his friends, and was manifest in the modesty, neatness, and integrity of his various communications to the public, he was an example which we grieve to have lost.

This chapter may fitly close with the tribute paid to the first Director of Harvard College Observatory by the official heads of the University — men who had known him long and well.

RESOLUTIONS OF THE CORPORATION OF HARVARD COLLEGE ON THE DEATH OF WILLIAM CRANCH BOND.

At a special meeting of the President and Fellows of Harvard College, in Boston, February 12, 1859. The President called the attention of the Board to the death of WILLIAM CRANCH BOND, Director of the Observatory and Phillips Professor of Astronomy, which took place on Saturday evening, January 29th. Whereupon it was

Voted, That the Corporation cannot record the death of Professor BOND without expressing their gratitude for his services to the college in building up the observatory, which has been under his care from the beginning, and in giving it an honorable name at home and abroad; their recognition of his distinguished ability as a practical astronomer, acquired amidst great difficulties and discouragements; and their profound respect for the excellence of his character in all the relations of life.

Voted, That the President be requested to communicate the preceding vote to the family of Professor BOND, with the assurance that they have the sympathy of this Board in their great bereavement.

CHAPTER II

GEORGE PHILLIPS BOND 1825-1865*

THE earlier years of GEORGE BOND are spoken of in the notes which follow from the hands of his daughters.

"We have few reminiscences of his early childhood, but I am told that he was peculiarly gentle and lovable, a tractable, intelligent pupil, in favor both with teachers and playmates. Sure I am that he never was guilty of a mean or underhand action, for he was the very soul of ingenuous truth. A quiet, reserved, self-contained boy, he, no doubt, did not easily make intimate friends, though he won the respect and the liking of all. He was keenly sensitive when the schoolboys laughed at his curling hair and called it 'girlish.' Though naturally grave and quiet, he had a fine sense of humor, and a large share of wholesome fun and frolic. He could never have been a dull boy in any sense of the word. One of his classmates recently said of him:—

"'GEORGE BOND showed in a very remarkable manner as a boy the same traits of character which distinguished him in manhood. He underwent no

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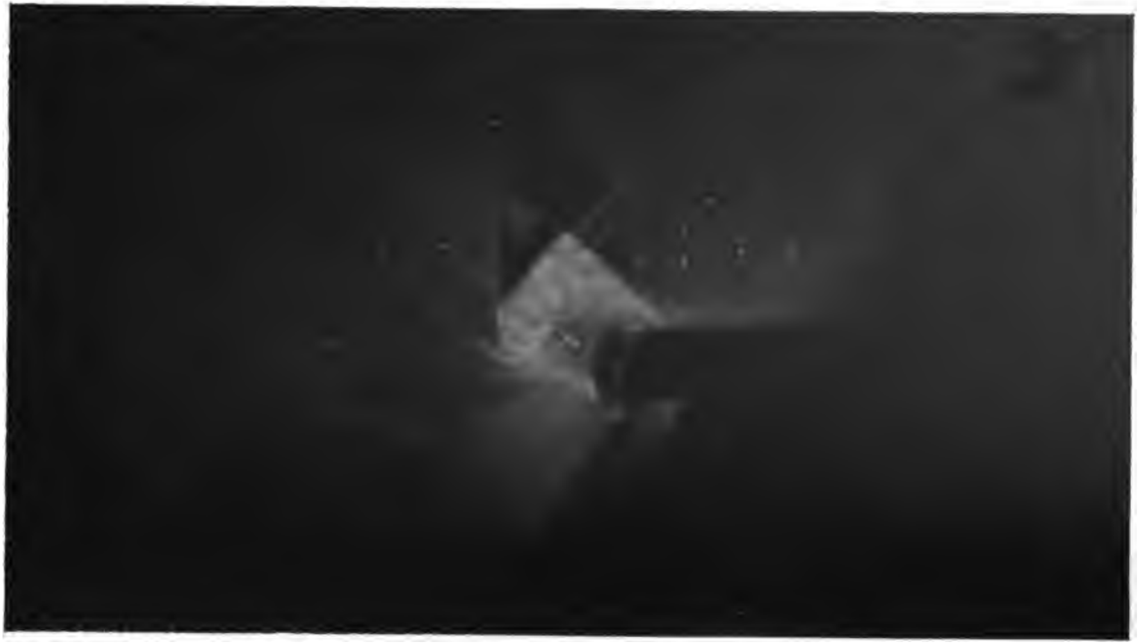
*GEORGE PHILLIPS BOND:—
Born in Dorchester, May 20, 1825; died in Cambridge, February 17, 1865. Married, January 27, 1853, HARRIET GARDNER HARRIS. Children: ELIZABETH LIDSTONE BOND; CATHERINE HARRIS BOND; HARRIET DENNY BOND (died in infancy).



THE NEBULA OF ORION.
DRAWN BY GEORGE BOND.

change, he simply developed. His untiring industry and perseverance, his fidelity and conscientiousness, were as apparent in boyhood as in maturity.'

"He was passionately fond of out-of-door life and sport, a true Englishman in his love of hunting and fishing. Until his health began to fail he went each year on some shooting expedition, either to Maine for deer and moose, or to the shores of Cape Cod for wild duck. He was deeply interested in ornithology, and when a lad had, for a time at least, contemplated devoting his energies to the study of some branch of natural history rather than to astronomy. His elder brother's death, however, left him no choice but to take that brother's place and to become the support and colaborer of his father. It was not without reluctance that he resigned his own special taste to turn his attention exclusively to the stars. So long as he lived it was his favorite recreation to read works on ornithology, or to watch the birds and note their plumage, song, and habits. He knew the notes of all our native songsters, and the haunts and habits of the wild creatures of the fields and woods. He could imitate so perfectly the notes of our birds that when he went among them they would gather around him fearlessly. He was a delightful companion for an out-of-door ramble. He noticed everything with quick, intelligent interest, from the drifting cloud to the blade of grass at his feet, observing each gentle curve and tender hue with the eye of an artist and the loving sympathy of the true poet's nature. He taught his children to love and care for all the small, timid, helpless creatures they found in their rambles through the neighboring fields. He encouraged us to watch by the hour the ants at their busy work; or the birds at their nest-building; to find on the snow the tracks of the field mouse and the wild rabbit; to gather nuts for the squirrels' winter store, and to listen in early spring for the first note of the bluebird and the robin. He was extremely fond of flowers, both wild and cultivated, and delighted in the care of an orchard of fruit trees, and in raising berries and vegetables, in which he had great success.

"He loved to be near 'dear Mother Earth,' as he used to say. Some of the sweetest memories of my childhood are connected with those happy hours spent in the garden or the fields with my father, for we were *always* with him. Poor man! I wonder how many moments he could call his own, free from our devoted attendance; but he was naturally fond of children, and showed rare tact in gaining their love and confidence. He was a strict disciplinarian, and exacted from us instant, implicit obedience; but I have never known any sympathy so tender and comforting as his. His presence was like sunshine. All who came into close touch with him must have felt the winsome grace of his familiar manner when he felt 'at home.' With his little girls he was the most devoted and delightful of companions, and the most charming of teachers. History and geography learned from him became as interesting as fairy tales, but I shall have to confess that he did not understand the difficulties of primary arithmetic. Most patiently he taught us the names and the positions of many of the stars and the constellations, and we were always shown anything of special interest in the skies. When a mere baby, not more than three years old, I can remember being held out of an open window in my father's arms—as far out as he could stretch safely—to see an eclipse of the moon. It was a winter's night, and very dark and cold, and I was quite as much alarmed as interested by the weird spectacle, so it made an impression on me.

"When a very young child I recollect that my father found in a field at a considerable distance from the house the nest of a ground sparrow, with the young just hatched. Every day until the birds were fledged he spared time to walk with me to the spot to feed the little things. This is merely one instance of the characteristic way in which he entered into the feelings and interests of a little child.

"He was desirous that Sunday should be not only a day set apart from others, but also the happiest in the week. Accordingly, after service he devoted his time chiefly to us, walked with us, taught us, and told us the most wonderful stories. He had a rare gift for 'making up' tales. With leisure to write them out, I believe he might have charmed the little ones as successfully as HANS ANDERSEN has done.

"The early death of his wife was a severe blow to his sensitive nature. She was a woman of a singularly sweet, gentle disposition, and their short married life had been very happy, though clouded by the shadow of her fatal illness. In the course of eleven months he lost his youngest child, his wife, and his father, and a serious fit of illness developed in himself the seeds of the disease which was to cut off his own life in a few short years. My mother died in December, 1858.

"In 1859, on the death of his father, he was appointed director of the observatory, and it was only then that the real difficulty of carrying on the work with the insufficient means at the disposal of the observatory became evident. The chronometer and clock business of the firm of WILLIAM BOND & SON was prosperous, and my grandfather had been able to supply any pressing need from his own purse. But my father had no private resources at his own disposal, and the sums supplied him by the funds of the institution or the liberality of a few Boston friends, were wholly inadequate to meet the wants of the observatory. Expenses were curtailed as far as possible, especially those of his own household, but the weight of care and anxiety pressed more heavily with each succeeding year. My father felt in honor bound to keep the work up to the highest standard, while the bitter jealousy and persistent enmity of certain disappointed candidates for the office he held left him no repose of mind or body. The outbreak of the war was a terrible blow to the progress of science, and for a time he was almost hopeless about the condition of the observatory. Money was scarce, and as none knew what a day might bring forth, donations toward astronomy were, of course, more scanty than ever. Still there were generous friends who gave ungrudgingly. Among them I should specially mention J. INGERSOLL BOWDITCH, the loyal, liberal-minded friend of father and son, Hon. JOSIAH QUINCY, ROBERT TREAT PAINE, and a few others.

"Of his own time, strength, and energy, my father gave without stint. He was willing to spend and be spent in a cause which seemed bound up with his honor.

"'When again and again warned by friends that fatal disease was approaching—or rather advancing—with hasty steps, and that the only remedy was rest, his answer was, "That is the only remedy I cannot use; I have a work to do, and must do it if I can, whether I am to live or to die;" and so, day and night, as the seasons passed, he gave more time and labor to his work than any well man should have given, and more than a sick man could give and live.'*

"Those were strenuous times, and in many a household pinching economy became the law of

* THEOPHILUS PARSONS.

necessity. In few, however, could there have been more quiet self-denial of personal comfort than was practiced by my father; and denial it was, for he liked luxury, and had a very fastidious taste. For instance, in those stirring days of battle he did not take a newspaper, but waited to hear the news from his brother, who lived near, or sent a messenger to read the bulletin. At one time even his cup of coffee was given up, and all articles of luxury, of comfort, and I fear often of necessity, were quietly relinquished. This rigid system of economy was, in part, forced upon him by actual straits of poverty, and in part conscientiously adopted that there might be some slender provision for the little children whom he was so soon to leave unprotected. He took a most despondent view of the political condition of the country after the beginning of the war, and feared we were on the road to anarchy, or possibly to a military despotism. The frail state of his declining health perhaps had this depressing effect upon his mind.

"No doubt his life was shortened by the privations and exposure forced upon him by the state of the country. The observatory was not properly heated, and the rooms he was obliged to visit were often bitterly cold and draughty. Only a few months before his death, when the committee was informed that the dome leaked so badly and was so damp that it was unsafe for him to observe there, funds for its repair could not be raised.

"With all his economy, he was ever generous and open handed. He gave not only out of the fullness of a kind heart, but as a principle of life, and was in the habit of systematically laying aside a certain portion of his income for the relief of those less blessed than himself. He faithfully put into practice a precept which you may have noticed in his diary: 'Let us not stop short with merely condemning a sordid spirit, nor yet content ourselves with being liberal up to the standard the world sets in giving, *but give till we feel we are losers by it.* And though we do all this, we must remember that we may give all our goods to feed the poor, and yet have no part in the Kingdom of Christ; may talk and write, as I do now, and yet be without God in the world.'

"My father was fond of music, and his nature was quickly responsive to all beautiful and lofty impressions. I know how deeply he was moved by the solemn sonorous music and the stately ritual of the English cathedrals. I think also he must have appreciated fine poetry, though I have but a dim recollection of his quoting passages from SHAKSPEARE, MILTON and KEATS. I distinctly remember his love for the grand Hebrew poetry of the Bible, particularly the Psalms and some of the Prophesies, and I know (as I have already said), that his love of the beautiful in nature was as poetical as artistic. In all nature he read a message to man; she was for him not simply the creation of God, but the manifestation of His very Being, the outward expression of His Power and Love. And so all the beautiful things of earth appealed to his sensitive, receptive spirit, not merely as pleasing to his sense of beauty, but as an inspiration to a deeper search for the hidden things of God's law, a constant revelation of the Majesty and the Love of God. His was a very reverent spirit, full of 'the fear of the Lord,' dwelling in very close communion with his Maker. Indeed, I think that the personal consciousness of the close presence of God grew to be the very essence of his daily life. When you read 'Brother LAWRENCE,' think of my father. He, too, walked as in the very presence of God. His simple Christian faith was something stronger than what we call a conviction. It was his very life. I do not believe he had ever known a doubt; his religion was as much a part of his nature as the breath he drew.

"He was scrupulously regular in his attendance at church, and for many years was active in parish affairs; was superintendent of the Sunday-school, and long held the offices of junior and senior warden. Of later years he retired from active work of this kind, but was never absent from service so long as his health permitted.

"My father had an intense love of color, apart from form, and delighted in the sky for its beauty quite as much as for the knowledge it held in store. On fair evenings in summer the whole family always went out of doors to a 'round stone' pier behind the observatory, and watched with him the glory of the setting sun and the gathering of the twilight gloom. It was an hour of pure enjoyment to him. I remember his talking to us about the certainty that color would be permanently photographed. He had some theories about it which I was too young to understand, and I have found no mention of them among his papers. He had an admiration, which in the last days became almost a passion, for gems. When he was very ill, some of his wealthy friends brought their jewels for him to look at, and dying as he was, the sight of their flashing colors seemed to afford him exquisite pleasure. Two or three weeks before his death the man who supplied us with ice heard of this fancy, and brought a huge block cut from the heart of Fresh Pond. It was as green as an emerald, perfectly clear, and glittered in the sunshine with every hue of the rainbow. Too weak to walk across the room, my poor father insisted upon being wrapped in shawls and carried into an outer hall, where it was so cold that the ice could not melt. He sat there by an open window for a long time, absorbed in the beauty of the sparkling colors of the block.

"He delighted in mountain scenery, and in the pure, dry air of high altitudes. Mountain air always seemed to give him renewed vigor of body and peace of mind. He had remarkable recuperative power, and his system responded so readily to a favorable environment that we have always believed that with proper care in a healthy place his life might long have been spared.

"Before his illness he traveled much among the White Mountains, visiting wild, unfrequented spots. He made maps of the region, which until recently were the standard authority for all the guidebooks of that section. The autumn before his death he passed several weeks in the western part of Maine, and feeble as he was, made several expeditions to explore the solitudes of that mountainous region. He was interested in calculating the altitudes of some of the peaks, and when we went for drives he often carried molasses, to be used instead of quicksilver, with some improvised instruments, for estimating the elevations. One of the White Mountains is called by his name. It is in a wild, solitary part of the mountains, seldom visited by tourists; but he used to go there, and loved its seclusion and the sheer, rugged precipices which distinguish it from other peaks in that group.

"My father was naturally very active and venturesome, and thoroughly enjoyed driving and riding, mountain climbing, brisk walking, and all manly exercises and games, such as boxing, wrestling, football, and the like. He was absolutely fearless, physically and morally. He always had the courage of his convictions.

"I suppose this fearlessness made him some personal enemies. Naturally he was gentle and peaceable, but he had a strong sense of justice, and when he felt it right to speak, his few words seemed to leave nothing unsaid. With all his self-control and gentleness, he could, on occasion, be extremely stern and severe; and modest and unassuming as he was, he yet had a perfectly just estimate of the value of his work and of his natural ability. His quiet modesty is so often referred to that you might easily believe that he underrated his own powers, but I think such was not the case. He had true, not false, humility, and knew that if his life were spared he could do 'something considerable,' as he expressed it. Ostentation and boastfulness were very repugnant to him, offensive to his taste as well as to his principles.

"He devoted about an hour daily to general reading. Works on natural history, travels and history were his favorites, I believe. He was particularly interested in church history, probably owing to his intimacy with Professor SOPHOCLES, who came once or twice a week to take tea with us. The two would sit beside the blazing wood fire in the dining-room discussing a subject not usually considered specially interesting.

"My father delighted in the society of intelligent men, and his few intimate friends were very dear to him. He went little into general society; but this was due to the pressure of circumstances rather than to lack of inclination. When abroad he greatly enjoyed his opportunities of seeing society, but at home he was closely confined to his post and rarely left it. I know he was a delightful companion, thoughtful and earnest, cheerful, and very original, with a quaint humor which was the more charming because he usually seemed grave. He was never dull or indifferent, but full of enthusiasms, and his interest once fairly stirred he could throw off his gravity and be very impetuous—

always within certain limits. He never lacked courtesy, I think. I am told that he never did a rude act, and I never heard him speak a *rough* word to any one. There were those, however, who did not understand him, and felt a restraint in his presence which they resented. No doubt he seemed a little aloof from the world, reserved and self-contained. Some people prefer the din and turmoil of the thronged city to the pure, serene silence of the mountain tops.

"In person he was rather tall (a little under six feet) and slender, becoming, of later years, painfully thin. His hair was wavy and very dark, if not black; his complexion pale, and his eyes of the deepest blue, with a glowing spiritual light in them that transfigured the worn face, lending it a singular power and beauty quite apart from mere regularity of feature.*

"Toward the end his physical suffering became intense, but he bore it with wonderful fortitude and patience and perfect resignation to a higher will than his. He was most anxious to live to complete his work on the nebula of *Orion*, being unwilling that it should be published in an unfinished form, without his own supervision. He worked upon it after he was too feeble to hold a pen, until the day before his death. With all his patience and courage, despite this incentive, he longed for the end. In those last sad days, when in weakness and suffering he lingered on the threshold of the next world, his frail body seemed a prison from which the eager spirit panted to be set free. The other life was very near to him. It was not the shadow of death that fell upon him—it was the brightness of approaching light.

"Remembering my father in those last years of his life, these words of Canon Liddon seem to

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*** There are no portraits of George Bond.**

describe the impression which his person and character have left upon my memory:—

"'It is said that the Roman conquerors carried in their faces the secret of the triumphs of an imperial people. Much more do sincere Christians walk the earth with the mien and bearing of a race of immortals; although the rays of spiritual majesty that stream forth from the burning spirit within often do but illumine the weakness of the body which yet encases it. Of such it is literally true that, whether they live, they live unto the Lord,—or whether they die, they die unto the Lord.'"

—*E. L. B.*

"Looking back upon the days of our childhood, the strongest impression that I have of my father is of one whose faith, whose love of God, was the ruling power of life. I think, children as we were, we realized that he walked as in God's presence, and that his life was a constant effort to do God service, whether in his work or in the harder task of bearing patiently, without complaint, long years of illness and suffering.

"He must often have talked to us, his children (far oftener than I now remember), of the faith and love that were the mainsprings of his life; but though I cannot recall his very words, I still remember, as if it were yesterday, the fervor and conviction with which he spoke. His faith was more real to him than his life.

"Perhaps it was because he felt and saw God in everything that my father's love of the beautiful was so intense, amounting almost to a passion. The wonders of the starry heavens; clouds piled in snowy masses against the deep blue sky; snow-capped mountains, pink in the last rays of the setting sun—these called from him such expressions of joy and wonder that I can remember now how he stirred in us also enthusiasm and awe. We were seldom in his presence at the time of sunset that he did not point out to us the beauty of the sight, and I remember how he tried to make me feel the grandeur of a thundershower as on a summer afternoon we watched one gathering in the west.

"I think he was always trying to make us see a deeper meaning in all around us, and when we brought our childish treasures to him to examine we were sure that not only would he appreciate them, but that he would tell us wonderful things about them, and send us away feeling that they were treasures indeed. A pretty pebble, or a little plant, became a wonderful thing to us after our father had examined it with us and talked to us about it.

"The happiest hours of our childhood were when our father played with us or told us stories. He had a most remarkable gift for telling original stories. Thrilling and wonderful, sometimes heartrending, we found them, and the very best ones he always told on Sundays.

"He must have taken his few spare resting moments to think of these stories for his little girls. The stories often had a purpose—a hidden meaning—but were so attractively presented that they did not seem to us 'tales with a moral,' usually so distasteful to a child, but rather inspiring or touching, as the case might be.

"Sundays were good days always for us children, for though the church service, to which father took us regularly, was long for little people, still were we not cheered by the thought that in the afternoon we should have our very best toys, saved expressly for Sunday use, and called 'Sunday playthings;' and would not father tell us a beautiful story, and find leisure to walk with us about the observatory grounds, look at our gardens, and perhaps have a game of play with us?

"How the weeds did grow in those little gardens! but we were taught that if we would have the pleasure of flowers we must have the toil of weeding the beds and training the vines. There were many moments when we wished we did not own a garden; but looking back, I can see the lessons my father tried to teach us as we sowed the seeds and dug in the little flower beds.

"He always allowed us to play in his study, even when he was at work on abstruse calculations, but this was with the understanding that we did not quarrel. His power of concentration was very great, and so long as we were good children we did not disturb him, even when playing or drawing pictures at one end of his large table. But the moment a discordant note was struck, little voices raised in dispute, he had to dismiss us from his presence. This we felt such a disgrace that I think we reserved most of our little differences to be settled out of doors, or in our own private playroom. My father's study I remember as a room very plainly furnished, the most noticeable thing in it being a large deal table in the center, covered with his papers. There he sat writing or figuring, hour after hour, while we drew and painted, or studied our lessons at one corner of the table, cleared for our use. There were some bookshelves and desks in the room, and the necessary chairs, but no luxuries, unless the open soapstone stove could be so considered. Plenty of sunshine streamed in the south windows, and even if the room was so plain and bare, I shall always remember it as the most cheerful place in the world. After my sister grew old enough to go to school, I well remember what a companion my father was to me; how he let me be in his room while he worked, and I played or toiled over the alphabet and very primary arithmetic, and how he allowed me to go with him around the observatory when he was examining instruments or directing his assistants. In the cold winter mornings, too, I spent the half hour out of doors with him while he chopped wood for exercise before going to his work. Even as a very sick man he persisted in taking what he considered the necessary amount of severe and vigorous exercise in the open air. All through his life he was very fond of out-of-door sports—riding, walking, etc.—and he did his best to encourage the same taste in his children, often for that purpose joining in our games of ball or tag, and thereby imparting very keen interest to our play.

"Very generous, often to the point of denying himself real necessities that he might give to others, he tried to teach us to love to give away our few pennies. Every Sunday morning he gave to each of us a cent; the one who had been the best girl during the week could put hers away in a pretty little white box, with a red rose on it; the other little girl put hers into a basket, from which she could take it at any time to buy with it candy or other things dear to children. But the penny that went into the white box was 'for the poor,' and happy was the little girl who had earned the right to place hers there.

"His life had to be one of great self-denial, and he met it bravely; nevertheless, he had a keen sense of enjoyment of comfort and luxuries, which he tasted generally only in imagination. His appreciation of fun and wit was very great, and though often reserved and quiet in company, he enjoyed to the full anything that was bright and lively. He loved to have young people with him in his hours of rest and relaxation, and he was particularly fond of young men, over whom his influence was very great. I think by his example, and by his inspiring talks with them, he helped to mould the lives of more than one of those who came in contact with him.

"Up to the very last of his life he continued to work, his heroic spirit and his firm faith supporting him to the end. His mental vigor remained unimpaired, and he continued at his labors till the day before his death. Though suffering physically, his spirit rose triumphant over all ills, and he passed, 'in full assurance of faith,' into that nearer communion with his Maker for which that faith and his life had fitted him."—*C. H. B.*

One of his life-long friends, Mr. Charles William Folsom, thus writes of him:—

"My earliest recollections of George Bond are connected with the old Hopkins Classical School, of which Mr. John B. Henck, a recent graduate of Harvard (1840), was preceptor. It is possible that I may have gone to school with George before that, but I do not recollect it.

"George Bond and myself, with one or two others, formed, as I might say, the senior class of the institution. We were fitting for college, and had the Greek reader, Virgil, and Colburn's Algebra as our food for the college requirements. George and I were very good friends, and used to get our lessons together; perhaps I helped him in the Latin and Greek; while he most certainly helped me in the algebra. He impressed me then with the same qualities that he showed through life—assiduity, modesty, gentleness, a high sense of honor, and a nameless charm combined of all these; and an underlying sense that in him would be found perfect firmness and devotion to the right.

"We were always excellent friends, and never had a cloud on our harmony. His younger brother

RICHARD was more vivacious. I do not think he had any great reverence for 'seniors' as such; and I think he and I had some tiffs on the football ground, always, however, amicably adjusted afterwards. He may have had a tiff or two also with Mr. HENCK, who was rather a martinet; but GEORGE never had any tiffs with anybody, scholar or master, and was very much liked by all the boys, and respected by Mr. HENCK.

"Mr. HENCK (who is still living, I believe) was well equipped for teaching, having been first scholar of his class; he was not exactly what you would call a lovable man, but he inspired respect. Respect, I would say, was inspired in those days (no longer, perhaps, with the rattan, but certainly) with the ferule. I seem to recollect the ferule being used three or four times in the school during that year, but not oftener. Mr. HENCK was an especially eminent mathematician, which must have made GEORGE very acceptable to him as a scholar.

"This year of school together lasted from August, 1840, to August, 1841, at which time we both entered Harvard College as freshmen.

"For the first year we saw considerable of each other, as I was living on Holyoke street and he at the corner of Quincy street. Here I made the acquaintance of EDWARD BROMFIELD PHILLIPS, whom GEORGE had probably known before, living, as he did, on the opposite side of the street. PHILLIPS was an extremely amiable fellow, but of a curiously awkward appearance. This made him sometimes a little of a butt with the unthinking, so I suppose he may have enjoyed the company of GEORGE and myself. No doubt the friendship formed by EDWARD PHILLIPS for GEORGE BOND at this time had a good deal to do with PHILLIPS' magnificent bequest of a hundred thousand dollars to the observatory not many years later.

"After graduation, of course, my opportunities of seeing BOND were less frequent, as I was rarely in Cambridge, and he as rarely left it; but I have the pleasantest memories of my occasional visits to his pleasant home, where I was always welcomed with the greatest hospitality. Others were more familiar than I with the salient points of his career as astronomer and physicist, with his sweet and unselfish example in church and home; but he will always remain to my mind one of the finest combinations of character, both gentle and noble, that I have ever known or known of."

Another college friend (Mr. SAMUEL F. COUES) writes:—

"My earliest recollections of my friend and classmate, GEORGE BOND, are of a quiet, reserved boy, not robust in health, though seldom, if ever, disqualified for his college duties. He led a studious, home life, and was not conspicuous in the sports or exciting incidents of college life. My proximity to him (alphabetically) led to an early acquaintance, formed in the class room, and I soon recognized his ability. He assisted me materially in the mathematical department, and always with such kindness that I became much attached to him. He was characterized by a peculiar sweetness of disposition; his smile and graciousness of manner I can never forget, nor my indebtedness to him for much needed assistance. He was uniformly gentle, courteous and affable, and under a calm exterior there was evidence of strength of character, firmness of purpose. He was conspicuously fond of nature, of natural objects. We occasionally walked together, and our talk turned in that direction and was apart from affairs of college. He was not a talker; but what he said was to the point. He was so modest and unassuming that his character and attainments were far less generally appreciated than they deserved. His life was largely within himself, yet he was a sympathetic and warm friend, one whom I never met without pleasure, and our friendship was never impaired in the least."

Another letter from a friend who loved him says:—

"He loved and was always very proud of his eldest brother WILLIAM, who he always declared had such genius, especially in the line of mathematics and science, that it would have been better for all the others to have died if he had been left instead. I cannot tell if it was true or not, for WILLIAM died when I was only five or six years old, and GEORGE was so unaffectedly modest that his belief did not necessarily make it the fact. WILLIAM was certainly a most brilliant scholar, and the action of the college in asking to let him be buried in the college lot, was intended to show their appreciation of him. What GEORGE and WILLIAM were in mathematics his father was in physics and his brother RICHARD in inventive genius; that is to say, the whole family was a remarkable one.

"GEORGE was for years superintendent of the Sunday-school and warden of Christ Church. He was faithful beyond words to any and every duty, and of a deeply religious nature, while his intense love of children would make the Sunday-school an attractive field of labor, except for the excessive shyness and reserve, which made any public position a heavy burden to him."

In September, 1856, the President of the United States (FRANKLIN PIERCE) appointed BOND to be Chief Astronomer of the survey of the north-west boundary between the United States and British Columbia. The salary was $3000, with expenses paid. After taking a few days for consideration, BOND declined the appointment on October 14th. From one point of view it may be regretted that BOND did not accept this very honorable and important office. His health would probably have been confirmed. His duties would have brought him into personal relations with men of intelligence and influence, who would have fully appreciated his very rare qualities and his great ability; and his own view of the world and affairs would have been widened. On the other hand, his father was well on in age, and BOND had been his mainstay for many years, and had, no doubt, the perfectly natural and justifiable desire to succeed him in time. He was devoted to his work, and happy in it, and a change would seem to him unfortunate. Perhaps if he had accepted this flattering offer, we should not now have his splendid memoir on the great comet of 1858, one of the most brilliant of his achievements.

A charming picture of the busy, interested and interesting life at the observatory—on its human and on its scientific side—is given by his sister-in-law, Mrs. RICHARD BOND, in the letter which follows.

"BOSTON, April 16, 1895.

". . . According to promise, I will tell you all I can understand about the observations of the sun spots. When I was a little girl I used to spend my August vacations at the observatory, and for several years in succession Professors W. C. and G. P. BOND were engaged in taking observations and drawings of the spots, etc., on the sun, using the five-foot equatorial that was afterwards placed in the west wing. The west wing was not built at that time, and the instrument was set up in a little house which we children called the 'Roundabout,' because, instead of the dome-shaped top alone, the whole building revolved around a large granite disk, upon which stood the pier of the equatorial. The last time I was at the observatory the granite block was still there, southwest of the observatory buildings, but I suppose it has been removed now to make place for the group of small buildings which Professor PICKERING has had put about there.

"In taking the observations, a wooden frame, which held a sheet of paper, was fastened, perhaps a foot or more from the eyepiece of the telescope, and so adjusted that the image of the sun, to which the telescope was pointed, should be thrown upon the paper. The image on the paper seemed, as I remember it, as large round as the tube of the equatorial, i. e. several inches in diameter, and one observer, with a sharp pencil, traced the spots as they were reflected on the paper, while the other wrote down any notes or observations, of time, or peculiar appearances, or explanatory of the drawings, or helpful in finishing up afterwards the sketches taken, necessarily, very rapidly. But both of them, besides being gifted with extraordinarily keen vision, had eye and hand and mind so thoroughly trained, that even to children it was fascinating to watch the certainty and accuracy of every touch, their enthusiasm and delight in the work, and the quick response and recognition of either to a remark or suggestion of the other. They never seemed disturbed by our presence (of course, we knew enough not to move or speak unless spoken to), and they often called us to notice anything of special interest, such as we could appreciate,—the size or shape of some particular spot, the shading of its edges, the change of its shape as it approached the limb, etc., etc. It is a wonder to me in looking back, to remember not only how constantly we were allowed to be with them, but how often they would take pains to show and explain to us such things as we could understand, really seeming to rejoice at our interest in them. You can remember GEORGE's love of children, and his bright, pleasant way with them. His father had the same. They never seemed tired of having us with them, or impatient of our questions, or vexed by any awkwardness or stupidity.

"In later years, when I was older, GEORGE often would come down from the dome to call us up to see some interesting or beautiful sight. I can remember, among others, some odd or specially pretty groupings of the moons of *Saturn* and *Jupiter*, *Venus* in the faintest crescent, in broad daylight, the sky of palest turquoise blue, and *Venus*, like a little silver boat, sailing in it. They were both remarkable for a kindliness and generosity which always impelled them to share their pleasures. . . . S. A. C. BOND."

What is said by Mrs. RICHARD BOND precisely expresses my own recollections of many visits, and of the extreme kindness of the astronomers to their young guests. I remember, as if it were yesterday, seeing the brilliant star *Alpha Lyræ* through the large telescope, and recall, if not the substance, certainly the manner of GEORGE BOND'S comments on what I was seeing. Everything seemed quite intelligible to me because it was so intelligibly and simply explained. This particular visit must have been previous to 1860.

Cambridge seemed a very learned place to the generation of boys to which I belonged. There was, first and most important of all, the observatory; then the Botanic Garden near it, with its wise and kind director, Doctor GRAY; the Nautical Almanac office was domiciled in a little brick building on the main street, and we all had a speaking acquaintance, at least, with some of the mathematicians; Admiral DAVIS'S house was full of curiosities brought home from his sea voyages; Professor AGASSIZ'S back yard was at that time occupied by delightful turtles, and entrance was free to all his young friends. He was very kind to small boys who brought him fossil ferns, etc., from their fathers' coal mines, I remember; and he always walked around our games of marbles on the earthen sidewalks. My recollection is, that the President of the College and Professor PEIRCE did not take this trouble, and that we children excused the lack of consideration on account of their high dignity and the abstraction which was suited to a great "geometer"—a word we had just learned.

The days at the observatory passed one like another. The nights were full of interest and of achievement. The whole history of BOND's short and busy life is written in the paragraphs of this chapter, and in the text of the memoirs which he printed. A list of them is given in an appendix, and some account of their contents in Chapter V. He was just launched in his career when he died, at the age of forty years. If his life had continued for a generation longer, it is certain that the quantity of his work would have been immensely greater. Its quality would not have fallen below the standards he had already reached. It should be mentioned that BOND was very anxious to secure for Cambridge the fine 18½-inch equatorial with which CLARK discovered the companion to *Sirius* in 1862. Negotiations for the purpose were in progress when it was bought by the Dearborn Observatory, then at Chicago. The failure to secure this fine instrument was a great disappointment to him. He visited Europe in 1863 to report on the cost of a large telescope for Cambridge.

Bond made two voyages to Europe, in 1851 and 1863, and extracts from the diaries which he kept are given in the following chapter.

No proper estimate of Bond's career can be formed that does not take into account the fact that, during the last third of his life, he was subject to the unrelenting enmity of two prominent American men of science. They and their friends were able to hamper him in many material ways; and he felt their hostility keenly. In these pages I have omitted every line referring to such subjects which could be left out with justice. But it is necessary to make this passing reference to a state of feeling which existed. His nature was serious and grave, and he held to his moral standards with a fidelity which he perhaps exacted too rigidly from others whose standards were very different. He was absolutely without fear, but could be harassed by small things. During many months his health was failing; during the whole of his career he was overworked in his science, and concerned about pecuniary cares for his family and for the observatory. It is impossible to understand him as a man without taking this constant and irritating opposition into account.

From my perusal of his entire existing correspondence, I have gained the impression that his uncompromising sense of truth and justice led him, at times, to be something less than perfectly just to others when their natures were utterly different from his own. He had ample provocation. There is not a shadow of doubt that he was perfectly satisfied in his own mind of the entire justice of the course of action which he consistently followed. At the beginning of his service as director he made a manly and generous effort to compose his difference with PEIRCE,* but failed in the attempt, through no fault of his own. In another relation he rejected an opportunity of compromise which presented itself, and there is no question but that he felt it a plain duty to do so, though another man, in his place, might have done differently. The criticisms of OTTO STRUVE on his father's observations on the *Orion* nebula gave the impulse which led to his own splendid work on the same object.

It is necessary to say a few words here in regard to the National Academy of Sciences and of its failure to include BOND in its membership.

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* See Chapter IV, letters of March 10 and March 12, 1859.

In the year 1863, on the proposal of several American men of science, of whom AGASSIZ, BACHE, GOULD and PEIRCE were the most active, the Congress of the United States incorporated the National Academy of Sciences. This body was "to consist of not more than fifty" members, to have power to make its own rules, and to fill vacancies in the membership. The selection of names for the incorporators was made by a few persons, and BOND was not invited to be one of the original fifty. Following the model of the Paris Academy of Sciences, the National Academy organized *sections*, as follows:*—

CLASS OF MATHEMATICS AND PHYSICS.

SECTION I—MATHEMATICS.

J. G. BARNARD,	WILLIAM CHAUVENET,
H. A. NEWTON,	BENJAMIN PEIRCE,
THEODORE STRONG,	JOSEPH WINLOCK.

SECTION III—ASTRONOMY, GEOGRAPHY AND GEODESY.

STEPHEN ALEXANDER,	ALEXIS CASWELL,
J. H. C. COFFIN,	CHARLES H. DAVIS,
J. M. GILLISS,	BENJAMIN A. GOULD,
ARNOLD GUYOT,	LEWIS M. RUTHERFORD,

JOHN RODGERS.

**Annual* of the National Academy of Sciences for 1863.

Other astronomers and geodesists were placed in other sections, as, *Physics*—A. D. BACHE and W. H. C. BARTLETT; *Mechanics*—J. E. HILGARD, JOSEPH SAXTON.

There is no question whatever that BOND'S name should have been included among the nineteen names of American astronomers and geodesists just enumerated. His place was in Section III—*Astronomy*, *Geography* and *Geodesy*—though his contributions in mathematics were considerable, as compared to those of some of the members of Section I. He was still a young man—thirty-eight years of age—but there were younger men than he in the list. There are certainly not more than half a dozen names among the nineteen just mentioned which stood for anything like the achievement and capacity which had already been manifested by BOND at the date of the organization of the academy. This is perfectly patent to all at the present time, and, of course, it was perfectly well known then to the director of the chief observatory in the country and to the professor of astronomy in its oldest university.

It was felt by BOND and his friends that his name had been omitted through the influence of a very few personal enemies, and there is no doubt that he was justified in so thinking. His relations to the Academicians, as such, were never cordial. He was not a member when he died in 1865, though there is no question whatever that, had he lived, his colleagues would have selected him for the honor at an early day, as they did other prominent men of science who had been passed over in the first selection. He was an honored member of the American Academy of Boston, a corresponding member of the Royal Bavarian Academy of Sciences of Munich, a Foreign Associate of the Royal Astronomical Society of London, etc.

At its February meeting of 1865 the Royal Astronomical Society of London formally awarded its gold medal to Bond for his splendid memoir on the great comet of 1858, which is published in Volume III of the *Annals* of the Harvard College Observatory, and for his other works. The official notification of the award reached Cambridge a few days after Bond had succumbed to the wasting disease from which he had suffered so long.* But his friends in London had pri-

* Bond's last observation was on August 24, 1864 — the occultation of β *Scorpii*. From this time until his death, his whole energy was given to the preparation of his memoir on the great nebula of *Orion*, which he did not live to finish.

vately informed him some weeks previously that the award was to be made.

At the request of his daughters, Professor HALL, for five years an assistant in the observatory, has written the following paper. Professor HALL'S memorandum is interesting in a double sense, as it throws light not only upon the history of the observatory under the BONDS, but upon his own history as well.

MY CONNECTION WITH THE HARVARD OBSERVATORY AND THE BONDS—1857–62.

While teaching school in Northern Ohio in 1856–57 I decided to undertake astronomical studies, and corresponded with Professor W. C. BOND of the Harvard Observatory, and with Doctor BRANNON of the Michigan University. From both I received kind letters, and Professor BOND held out the inducement of a small salary for assistance in his observatory. My wife and I reached Cambridge in the last part of August, 1857. We had a kind reception from Professor W. C. BOND. Professor G. P. BOND was absent on a visit to New Hampshire. I was set to work making observations for time, and was shown how to use the transit circle, to read the chronograph sheets, to work out the instrumental constants, and to compare and rate the chronometers. Professor BOND was very kind and pleasant, so that under his guidance I made good progress. I worked hard, and spent most of my time at the observatory. After a month or six weeks Professor G. P. Bond returned. He seemed a little surprised to find an assistant in the observatory, and doing so much work. He had a free talk with me, and found out that I had a wife, twenty-five dollars in cash, and a salary of three dollars a week. He told me very frankly that he thought I had better quit astronomy, for he felt sure I would starve. I laughed at this, and told him my wife and I had made up our minds that we were used to sailing close to the wind, and felt sure we would pull through. He appeared satisfied. Afterwards I worked a great deal with him as an assistant for recording and reducing his observations.

Professor W. C. Bond was in poor health when I entered the observatory, and he died early in 1859. He had shown me how to manage the transit instrument, the clock, chronograph and battery, and how to handle and compare watches and chronometers so as not to disturb their rates. I think he had not received a special training in astronomy and mathematics, and that his knowledge of theoretical astronomy was not extensive; but he was very skillful in the use of instruments, and was a careful and excellent observer. He was a kind and conscientious Christian gentleman.

Professor George P. Bond succeeded his father as director. He was very active during my stay at the observatory in making experiments and observations in photographing the stars, in photometric observations, and in his work on the nebula of *Orion*. His work on the Comet of Donati, in 1858, was a very complete investigation of the physical appearances of that great comet. I assisted Professor Bond in all this work and in the reductions, besides pushing on my own studies. I have a very distinct recollection of how cold my feet were when he was making his winter observations on *Orion.* I sat in the small alcove of the great dome behind a black curtain, and noted on the chronometer the transits of stars when Professor Bond called them out, and wrote down also the readings for declination. For some of the brighter stars which were observed on the chronograph I had to note the click of the key, and my record was compared with that of the chronograph down stairs. I became so expert that the difference rarely exceeded a tenth of a second, and for the fainter stars the chronograph was not used. Sometimes I was called to the telescope to examine a very faint star, or some configuration of the nebula. Professor Bond had one of the keenest eyes I have ever met with. His work on this great nebula forms an epoch in its history.

His experiments in photography were among the earliest in what has now become such an important branch of astronomy. Professor Bond indulged great hopes that photography would render much aid in the measurement of double stars and clusters. We made and measured a great number of photographs of *Mizar* (see *Astronomische Nachrichten*, Vols. 47 and 48).

In his photometrical work, Professor Bond compared the light of the sun and moon with that of the planets and several stars. He was very ingenious in his photometric methods, and in carrying them out to get results.

Professor George P. Bond had received, evidently, a much more complete training than his father. While he had not that familiar knowledge of mathematical formulas which distinguishes the professional mathematician, he had what is better: He was thoughtful and ingenious in his investigations. He liked to study things in their actual relations, and had the spirit of an inventor. His style of mind led him to original work. He was the first to apply the method of mechanical quadratures directly to the rectangular equations of motion, a method afterward discovered and elaborated by ENCKE. He was among the first to take up photography and carry it out to practical results. His ability has not, I think, been sufficiently recognized; but he was a shy and reserved man, made so, perhaps, by the condition of his health.

Although I was poor and worked hard, I was not sick a single day during those five years in Cambridge. They are for me a pleasant remembrance of hope and struggle, and I was fortunate in having to deal with two such honorable men as the BONDS. ASAPH HALL.

WASHINGTON, 1895, March 20.

OBITUARY NOTICE OF GEORGE P. BOND.*

"GEORGE PHILLIPS BOND, the worthy successor of his father, the first director of the Cambridge Observatory, died of consumption on the 17th of February last, a few months less than forty years old. He was born at Dorchester, on the 20th of May, 1825, and was graduated at Harvard University in the summer of 1845. He began to make observations as early as the year 1842, at the temporary observatory on Quincy street, where he soon took the place of his deceased brother, W. C. BOND, Jr., a young man of high promise as an astronomer. After graduation, and upon the permanent establishment of the observatory, GEORGE BOND took the post of assistant

* From the *Proceedings* of the American Academy of Arts and Sciences, May 30, 1865.

observer, which he held until 1859, when, upon the death of his lamented father, he was called to succeed him as director. Thus his whole life, even from boyhood, was devoted to astronomical labors in connection with the observatory which the BONDS, father and son, have made illustrious. Upon these devolved the heavy task of organizing the new establishment, and of carrying it on with means in slender proportion to its work. Suffice it to say that the high position which the Observatory took under the direction of the elder BOND was maintained under the younger. To its interest, and to scientific labors in connection with his official duties, he gave himself, from first to last, with entire devotion.

"The most important of his scientific investigations are:—

"1. Those relating to the mathematical theory of some portions of astronomy, especially his papers on "Cometary Calculations," and the "Method of Mechanical Quadratures" (in which he anticipated a valuable improvement afterwards given independently by ENCKE), and that on the "Use of Equivalent Factors in the Method of Least Squares." To this category belong in part his investigations upon *Saturn's* rings, which form the first step toward the present state of the problems connected with that system.

"2. The reduction of the observations made for the United States Coast Survey Chronometric Expeditions between Cambridge and Liverpool, effected under his direction. Upon these depend the most trustworthy American longitudes.

"3. The observations of zones of small stars. He mainly prepared the plan for observing and reducing these zones, and even graduated the mica scales employed in them. He also made many of the observations—most of the published portion, and a large share of those still in manuscript.

"4. The two works on the comet of DONATI and the nebula of *Orion* are, however, his most important contributions to astronomical science. The former commanded the general admiration of astronomers, as a full and faithful monograph of the physical phenomena of that celebrated comet, and was crowned with the highest honors of the Royal Astronomical Society in the recent award of its gold medal. The latter work—understood to be complete as to the researches, but the exposition of which his waning strength did not permit him to finish,—it is hoped may, in the main, yet be given to the world, to vindicate the essential accuracy of his father, and to reflect additional lustre upon his own memory. The spirit and personal characteristics of Professor BOND may be expressed in few words. He was a remarkably patient and industrious, as well as skillful, investigator, and was ever anxious that his works should possess the qualities, now so imperatively demanded, of unity and completeness. That he did so much for science, and did it so well, during the few years that were allotted to him, must have been partly owing to an extreme reluctance to dissipate his powers by beginning new works while old ones were still unfinished. Science to him was not a pastime, but a serious calling, to be pursued with the utmost conscientiousness and singleness of purpose. These characteristics were strikingly exemplified in the latter months of his life, in the perseverance and earnestness with which, under great physical weakness and constant suffering, he continued to the last his work upon the nebula of *Orion*, dictating to and directing his amanuenses long after the strength to write was gone from him. He was ever watchful of the interests of the institution committed to his charge, and strict in the performance of every duty. All who knew him well can testify to the great simplicity, purity and uprightness of his character. He was deeply penetrated with the spirit of Christianity, and was a devout member of the Episcopal Church.

"He visited Europe first in 1851, and again in 1863. In 1851 he observed the total eclipse of that year in Sweden, and visited the principal observatories of the North of Europe. In 1863 he spent a few months in England and Germany, renewing personal intercourse with old friends and correspondents, and forming new ties, too soon, alas! to be severed."

Among the obituary notices of BOND, the following paragraphs from the *American Journal of Science* (written by Hon. WILLIAM MITCHELL, chairman of the Board of Visitors, H. C. O.), are selected for printing:—

"It is seldom that astronomical science has received a more severe blow than that occasioned by the death of GEORGE PHILLIPS BOND, of Harvard College, Phillips Professor of Astronomy, and director of the observatory connected with that institution. After a lingering illness of more than a year, during which his ardor in the study of the heavens led him oftentimes to exposures entirely incompatible with the state of his health, he closed a useful and unblemished life on the 17th of February, eight days after his compeer, Captain GILLISS.

"As an accurate and truthful observer of astronomical phenomena, he was, without question, unequaled by any one in this country, and among the first in the world. In his short career he contributed many valuable papers of original discoveries and calculations to various periodicals and institutions in this and other countries.

"His greatest work, and that which gave him honor the world over, is his account of the Donati comet, which constitutes the third volume of the *Annals* of the observatory. To this the palm of unrivaled excellence has been freely awarded by the astronomical observers of Europe. Well trained by his lamented and distinguished father, and taking advantage of the best telescope mounted in so high a southern latitude, he explored with searching scrutiny the great nebula of *Orion*, a work which he pursued with untiring zeal and anxiety in his latter days; and while we fear his waning strength may have left it incomplete in form, we are assured, and rejoice in the assurance, that abundant ability remains in the observatory to prepare it for publication.

"We might dwell much longer on his astronomical history, but the necessary brevity of this notice requires that we should turn to his private life. It is rare, indeed, that so many virtues are blended in any man. His innocent, unpretending manners, his perfect absence of vanity or pretension, crowned with an unwavering Christian faith and deep sense of religious obligation, secured for him, not the mere respect, but the kindest regard of all who had the happiness of his acquaintance."

The Board of Visitors to the observatory made its annual visitation on November 30, 1864, and prepared its report for signature. Before the report was signed, Bond had died at his post. In an addendum to their report, the Visitors say:—

"On the occasion of our official visit in November, his usual report was not prepared; the state of his health had rendered it impossible. A sort of brief of what the report was intended to be, was before him, when he gave verbally, and with intense anxiety, a minute account of the transactions of the preceding year. It will always be recollected by the committee as a most impressive occasion.

"No possible doubt existed in the minds of any one of them that he was listening to the last official accounts of their inestimable friend; and they now bear willing testimony to the ability and fidelity with which he constantly performed all the duties of his office; enlarging, strengthening and perpetuating the admirable work of his lamented father; giving to the observatory a high rank among kindred institutions in this country and in Europe, and contributing, in no trifling degree, to the elevation and honor of Harvard College. . . . Although it is strictly the province of the committee to speak of Professor BOND in reference to his official character, they feel bound to allude to his private life and moral worth, to those qualities of the heart that specially endeared him to all who had the happiness to know him. With the name of GEORGE PHILLIPS BOND will always be associated *goodness* as well as *greatness*. During the lifetime of Professor BOND, your committee were oftentimes restrained in the full expression of their conviction of his great usefulness, lest it might savor of adulation; but that restraint no longer exists, and they can freely say that his short life was an ornament to the moral, and a shining light to the scientific world."

At a meeting of the wardens and vestry of Christ Church, Cambridge, the following resolutions were unanimously adopted:—

WHEREAS, It has pleased Almighty God to call home the soul of our beloved friend and associate

GEORGE PHILLIPS BOND, late a warden of this parish:—

Resolved, That we, the wardens and vestry of Christ Church, for ourselves and on behalf of the fellow-worshipers of the deceased, desire to testify our deep appreciation of his exceeding nobleness and purity of character, his absorbing and unselfish devotion to duty, his unobtrusive Christian humility and self-abnegation;

That we recognize in the life and labors of Professor BOND an admirable union of wide and accurate scientific attainments with the most absolute and implicit Christian faith, extreme modesty and ingenuousness of disposition, and a prompt and glad recognition of merit wherever and however displayed.

CHAPTER III

EXTRACTS FROM THE DIARIES OF GEORGE BOND DURING TWO VISITS TO EUROPE — 1851 AND 1863

EXTRACTS FROM THE DIARY OF 1851.*

BRENT, DEVONSHIRE, June 2, 1851.

I RECEIVED to-day a card of invitation to visit Greenwich Observatory with the Committee of Examination, on the 7th. Letters also arrived from Messrs. HIND and SHEEPSHANKS, a pamphlet from Lord ROSSE, cards from the American Minister and Mrs. RUSSELL STURGIS, etc.

LONDON, June 4th.

I went to-day, for the second time, to the great exhibition, with RICHARD [BOND]. Anyone who is not satisfied would better find another world to live in than ours, where the art of man can go no farther than it has done in the contents of the Crystal Palace. To examine the articles is hardly a greater entertainment than to see the vast interior filled with people, not crowded, but walking about in all parts. The effect of the trees standing in the

* At this time BOND was twenty-six years of age.

inside of the building is strikingly beautiful. I stayed there from 10 A. M. to 4 P. M. A month of such days would carry one pretty well through with it. Be as fastidious as you please, you may pass by one section after another by the hour, heedless of their contents, and yet at the end of that time you will be at some place where you may revel in the new and interesting scenes still surrounding you,—different from, but not less beautiful than, those you have passed. It is the most luxurious kind of sight-seeing ever imagined. The Arabian Nights are thrown far into the shade by the realities of the Crystal Palace.

[LONDON], June 7th.

I went, yesterday, to the observatory at Greenwich, and there met Lord ROSSE, Professor QUETELET, M. MATHIEU, the Dean of Ely, J. C. ADAMS, Mr. SIMMS, Mr. JOHNSON of Oxford, Sir J. HERSCHEL, Mr. AIRY, Mr. MAIN, Mr. SHEEPSHANKS, Mr. HIND, Sir JOHN LUBBOCK, Professor MILES, Doctor WHEWELL, Mr. DAWES and Colonel SABINE. There were nearly a hundred present. The new zenith sector had just been mounted. The wires are in front of the object glass, together with the frame which carries them, and a prism to reflect the rays to the eyepiece. The rays pass from an object near the zenith, through the object glass, are reflected at half the distance to the focus from quicksilver;

back through the object glass to the prism in front, thence at right angles to the eyepiece. The transit through the field takes thirty seconds.

For the application of the electro-magnetic method to the new transit circle, the break circuit key is on the telescope close to the eye end.

At the dinner about forty were present. A white-bait dinner—turbot, salmon, etc.—from six to ten o'clock. I notice that the most distinguished scientific men are bad, hesitating speakers, except, perhaps, Sir John Herschel. Doctor Lloyd, Mr. Airy, Mr. Adams, etc., are instances.

Paris, June 23, 1851.

After French and breakfast I took a cab for No. 7 Rue St. Thomas d'Enfer, to call on Leverrier. He was at home, and I sent my letter of introduction by the servant. Presently I was in the presence of the great savant. In appearance he is decidedly the man I should have wished to find him; but most unlike a Frenchman. He has an undeniable Anglo-Saxon face, or Norman, rather, which he calls himself. He is from Cherbourg. He is well made and good looking, with light hair, about thirty years old, or less.* In the frankness of his manners, in the absence of all *assumed* dignity, and in other points of resemblance, he is not unlike Mr. Adams.

* Born at St. Lo, March 11, 1811.

His straightforward way of expressing himself, and absence of all ostentation render him what I should call accessible. The moment we met my first impression was how much more he is like an American or an Englishman than a Frenchman. He speaks English about as well as I speak French, but we had not much trouble in understanding each other. He gave me a ticket for entrance to the National Assembly, of which he is a member. Finding that I had not been to the observatory, he proposed to take me there, first giving me an invitation to his house on Wednesday next to meet M. Faye. We went to the observatory together, and there met M. Faye and M. Villarceau, stayed there about two hours, and then left to go to the Assembly with M. Villarceau, thence to the meeting of the celebrated Academy of Sciences. I went to the Institute later. The proceedings were admirably conducted in an appropriate and dignified manner. The hall where the sessions are held is wainscoted with oak, and has a fine oak floor. The walls are ornamented with pictures, busts, and statues of distinguished men. The number of persons present was perhaps two hundred.

As M. Leverrier did not reach the meeting till it was more than half through, M. Faye presented the daguerreotype of the moon and the engraving of *Saturn*. The announcement of the first produced a sensation—such M. VILLARCEAU assured me—as he had scarcely ever before witnessed at a meeting of the Academy. Soon after M. LEVERRIER entered, and instead of taking the highest seat, he came and sat by me. I am sure from the pains he took to get there that he intended it as a compliment, and I certainly esteem it a very high one.

June 25th.

At half past eight last evening I went to M. LEVERRIER'S, and there met Messrs. FAYE, VILLARCEAU and FOUCAULT. The latter is a very small man. He says he derived the first impression of his celebrated experiment three or four years ago on considering the motion of the centrifugal pendulum, while engaged in attempting to obtain a regulator for uniform motion.

M. LEVERRIER took me to his study, where his great calculations have all been made. He showed me his unpublished works,—an immense mass of calculations relative to comets; a new theory of the sun, carrying the perturbations to the thousandth of a second. This theory has not yet been compared with observations. To do so he has formed anew a fundamental catalogue of stars. He spoke of *Neptune* and of Professor PEIRCE. That the remarks of the latter have irritated him to the last degree is plainly evident, and much to be regretted. His impressions of Professor PEIRCE'S position are, I infer, taken from the *Sidereal Messenger*, as he continually confounds what Professor PEIRCE has written with what Professor MITCHEL has written, always mentioning the two names together. He showed me all his original computations for the discovery of *Neptune*, and selecting the portion including the discussion of the limits, he pointed to it, saying that if it had not been for that Professors P. and M. (always together) would have had nothing to say. He took pencil and paper to show, how between the distances of 30 and 37,* there was a point where the formulæ became indeterminate, yet that this was an indeterminateness of form only, and to illustrate this he assumed a term in the perturbation to be represented by $\frac{R}{a}\sin(at+b)$, showing that development in a series of powers of a to t^2, or including the terms of $a^2\,t^2$, avoided the whole difficulty. He then turned to page 159 of his memoir, and there showed that he had represented the same thing there. Here he said was the error of Professors PEIRCE and MITCHEL, for, said he, they had not noticed that the form only of the perturbation, and not the perturbation itself, passed through infinity. They could not have read his memoir, or else they did not understand it. His language showed how much his feelings were excited while dwelling on this topic.

* The sun's distance from the earth = 1.

The engraving of *Saturn* he is going to have copied on a large sheet to take to his lectures at the Sorbonne, where he is Professor of Theoretical and Practical Astronomy.

M. Biot was very desirous of having a plate of *Saturn*, which I shall send at the first opportunity. M. Foucault first conceived the idea of his experiment three or four years ago, but though convinced that there would be a time of rotation, it was not till recently that he recognized the effect of latitude.

Ipswich, July 3d.

To-day I addressed the British Association for the Advancement of Science, the matter having been arranged yesterday by Mr. Whewell, President of the Physical Section, Mr. Airy and Colonel Sabine. It was very good fortune to have the honor of explaining the apparatus for observing by electro-magnetism to H. R. H. Prince Albert. After I had read what I had intended for the occasion, the Prince, at the instance of Mr. Whewell, came to the apparatus and examined it with attention. His questions were intelligent, and he seemed readily to understand the design of the invention. He was desirous of knowing the process of reading off the results, and comprehended at once the way of effecting this by double entry.

In the course of the day Sir David Brewster and Colonel Reid advised strongly that [the chronograph] should be placed in the great exhibition, and offered every facility for getting it there. We shall probably enter it, as they all gave out strong hints that it would be well received. I exhibited the daguerreotypes of the moon.

LONDON, July 5, 1851.

Just after I had finished the explanation of the spring-governor [chronograph] and had sat down at the table to write an abstract, I was delighted to meet Dr. [ASA] GRAY, our next-door neighbor at home. [Dr. and Mrs. GRAY] are now living at Kew, and invite us to come there on Wednesday afternoon. Received invitations to go to Shrubland on Saturday, from Sir W. and Lady MIDDLETON, and from Lord WROTTESLEY, to visit him at his place near Wolverhampton. To-day we had in the same car with us the Duke of Argyle. I am satisfied that the officers of the association interested in astronomy have not allowed an opportunity to pass of assisting us in illustrating the operation of the new method of observing. In consequence of the earnest desire expressed by Sir DAVID BREWSTER and Colonel REID, who are on the Committee of the Great Exhibition for Philosophical Apparatus, we shall remove [the chronograph] to the Crystal Palace as soon as possible.

HAMBURG, July 14th.

To-day I went to call on M. RUEMKER, of the

Hamburg Observatory, which is connected with the Navigation School. He is between fifty and sixty, or sixty perhaps,* hesitates much in talking, but understands English and speaks it very well. He was very kind, invited me to his house, and, indeed, seemed very desirous of having me go there to stay. We went over the observatory, where I saw a transit by the former REPSOLD, now dead, and a transit-circle by his son, whom I saw at work with his dividing apparatus. In the afternoon I went to Altona, and saw Doctor PETERSEN and the observatory.

SCHUMACHER'S library is on sale, 5000 volumes, said to be in admirable condition, for 3000 rixthalers. I must see to this. Doctor PETERSEN tells me of several who are going to Gothenberg,†—AIRY, Mr. CARRINGTON of Durham, and Professor AGARDH of Lund. He therefore advises me to go to Kalmar or Carlscrona, where Professor OLUFSEN of Copenhagen is going.

Madame RUEMKER is English, and gave me much information as to the personal history of distinguished astronomers. HANSEN of Gotha is a very amiable character, was once a watchmaker; BESSEL was thin in person, hollow cheeks, etc., and worked himself to death; D'ARREST is very young; BESSEL

*** Born at Stargard in 1788.**
† To observe the total solar eclipse of 1851.

looked fourteen years for comets and found none, Doctor PETERSEN, fifteen years, at the end of which long period he told me he one night, after observing *Neptune*, passed his telescope over the sky for a minute and found one, and soon after, in a year or two, two more. I returned to the hotel at 10 P. M., REPSOLD accompanying me home.

Tuesday, July 15th.

I spent most of the day in company with M. RUEMKER and lady, M. REPSOLD and Doctor PETERSEN, and a Doctor or Professor JONES, a *literatus*. Mr. RUEMKER is one of the real hard-working men, and reminds me of father in some of his ways. Doctor PETERSEN I like much; he seems so perfectly simple.

Tuesday, July 17, 1851.

I tried to navigate the streets from the Dirchstrasse to the observatory, and at last succeeded. The first night I spent in Hamburg I acquired the erroneous impression that the sun set in the east. I cannot get rid of it, and my only method of finding my way is to go in the wrong direction, which is always right.

HAMBURG, July 21, 1851.

A party of six or eight Englishmen visited the observatory accompanied by Doctor PETERSEN. Four of them, among whom are Captain BLACKWOOD and Mr. HUMPHREYS, propose to go to Sweden to see the eclipse, to Carlscrona, Kalmar, or Christianstad, or in that region. At 7 P.M. came Mr. HIND and Mr. DAWES, also on their way to Sweden, but not decided as to where to go. Wishing, however, to reach Christianstad, or Kalmar, and thence the interior, they are at the eleventh hour. I was busy to-day in packing, for the telescope which I take with me in the trunk occupies more than all the spare room.

Friday, August 8th.

At about seven I went on board the transport steamer to take us from Cronstadt to St. Petersburg. A cold, comfortless passage up the Neva to the customhouse pier; here we waited half an hour for our passports. But I met Mr. ROSENCRANTZ of the observatory at Poulkova, most kindly sent by M. STRUVE to conduct me directly to Poulkova. This was a great relief to me, and I cannot be too grateful for the kindness. . . . A carriage was ready for us in the street, and before noon I was at the observatory. I am truly grateful to M. STRUVE and his son for the kindness they have shown to me. The former, in the course of the afternoon, explained to me the method of using the great prime vertical transit, his favorite instrument. In the evening I observed, in company with M. OTTO STRUVE, with the great refractor. At that time the atmosphere was not favorable. Later in the evening there was a decided improvement, but having been awake at

3 A.M., and that almost without previous rest, I was quite tired, and went to my room.

POULKOVA, Sunday, August 10, 1851.

I awoke very late this morning, it being past nine when I got up. I am pleased to see here more regard paid to the day than is usual, I think, on the continent. Most of the family went to church. At dinner there was a large family party. The fruit and vegetables are supplied from the gardens of the observatory. The raspberries in the garden where I was this morning are the finest I have ever seen.

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POULKOVA, August 11, 1851.

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In the evening I observed, with M. OTTO STRUVE, with the great refractor until past midnight. He detected a companion of β *Aquilæ*. I am entirely satisfied with the performance of the great refractor.

POULKOVA, August 12, 1851.

I witnessed to-day the process of level-making, which is carried on in the atelier of the observatory to great perfection.

. . . .

In the afternoon I went to the prime vertical room, and saw M. STRUVE make the complete observation of a passage of ι *Draconis* east and west, in the four positions of the instrument. This is one of the stars (three) employed in the series now in progress for determining nutation.

. . . .

I saw ι *Draconis* with the prime vertical instrument most excellently defined.

In the evening I observed till midnight, with M. OTTO STRUVE, with the great refractor. The vision was not good, but the appearance of the stars entirely similar to that in the Cambridge refractor. But I have not yet had a favorable night. The effect of an unsteady atmosphere on close double stars is precisely as with us. Looked at λ *Cygni*, dist. 0″.5. I could with some difficulty determine its position 124°. M. OTTO STRUVE gave 116°, and appeared, from greater experience in double star observations, to see it more easily, and to observe the measures of position and distance much more quickly than I.* By practice, he has acquired a facility in steadying the motion of the telescope with his finger so as not to be so much incommoded by the irregularities of the clock as those less in practice. The going of the Munich clocks, compared with that at the Liverpool Observatory, is insufferably bad. I speak as well of ours at Cambridge as of that at Poulkova, but the latter may be scarcely as regular as the one at Cambridge. Both have great imperfections. α *Lyræ* has the same color of

* OTTO STRUVE's place for 1851-57 is $p = 103°.1$; $s = 0''.53$.

brilliant blue or violet as always seen; it must belong to the star. At present the most difficult double star is No. 365 of O. Struve's catalogue, or 3130 Σ, dist. 0″.25.

. . . .

There are, besides M. Otto Struve, two assistants in the dome; one soldier-servant to turn the dome, etc., and one assistant to record.

Pulkova, Wednesday, August 13th.

This forenoon I was in the apartment of M. Struve, and saw him go through the process of computing the observation made yesterday on *ι Draconis*. I have the original computation to take home with me. The precision of the results is nothing short of wonderful. Yet one must be convinced that tenths of a second of arc take the position here that seconds have hitherto done elsewhere.

. . . .

With the prime vertical instrument three stars only are observed in the year, and one only at a time, according to the season. Eight complete determinations of each in a year are considered sufficient. I recorded with M. Struve his observations for this day's passage of *ι Draconis*. The image of the star I saw, as on yesterday, most perfectly defined—power 270 or 300. I think better definition I never saw.

Yesterday I heard from M. Struve an anecdote about Sir W. Herschel's marriage, which this memorandum will be sufficient to remind me of. He has a profound respect, even admiration, for the works of Sir W. Herschel. It delights me also to hear him speak of Airy and Sir J. H., and of their private characters especially. Airy knows many of the English poets by heart and Schiller also. Neither Bessel nor Hansen received a university education, and Encke did not graduate, being obliged to leave by reason of war. Hansen is regarded as the first theoretical astronomer living. Knorre of Nicolaief stands very high. Dent's clocks are preferred at Poulkova to Kessels', and his chronometers are thought to be much the best.

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Poulkova, August 14, 1851.

In my morning walk after breakfast with M. Struve I was greatly interested in his account of the grand works now in progress in the observatory. First, the fundamental catalogue of 500 stars, on each twenty-four observations by transit, six in each position, and then the object [and eye ends of the instrument are interchanged]. Then the catalogue by the meridian circle, to be [reduced] differentially with the fundamental stars. M. Otto Struve has determined the constants of precession.

In the afternoon I went, as on yesterday and the day before, to the prime vertical room with Professor STRUVE to observe the passage of ι *Draconis*. I have gone through the process of leveling to-day, and I am assured by M. STRUVE that I am the first who has done so since the instrument was mounted in 1839, twelve years ago.

. . . .

Early in the evening I went to the dome with M. OTTO STRUVE, and observed till 3 A. M. Besides double star observations (one favorably situated we agreed precisely in), we saw BRORSEN'S new comet; then *Neptune's* satellite, on which we made a good series of observations: O. STRUVE, position 30°, distance 14″.7; G. P. B., position 28°, distance 15″.7, about, as near as I now remember. Afterwards I proposed to look at *Saturn;* before leaving the dome we did so. O. STRUVE inspected it first, and I perceived instantly that he was seeing the new ring for the first time, and with entire certainty. I suspected so before he spoke.

Friday, August 15, 1851.

I went to bed at 3 A. M. It was already strong twilight. After breakfast M. STRUVE and OTTO STRUVE had a long discussion on the new appearances about the ring of *Saturn*. We went to the library, and all the works bearing upon the subject were referred to. Professor S. is decidedly of opinion that the ring is in process of change, the width increasing, as he said immediately on referring to our engraving, compared with those of Sir W. HERSCHEL and the older astronomers. Indeed, the fact seems beyond question, and is in strong confirmation of the theory that they are in a fluid state. The manner in which the discussion could be made here shows the value of an observatory library. In a quarter of an hour we were in possession of a great mass of evidence on the subject.

. . . .

M. STRUVE says that the strongest internal evidence exists in Sir W. HERSCHEL'S writings that he had prosecuted a search for *Neptune*, and missed it only because it was too far south in declination. He himself at Dorpat made an examination, with the same object in mind.

POULKOVA, Monday, August 18th.

. . . .

The history of a collection of works from Pekin, in China, now in the Poulkova Library, is quite singular. In the time of PETER the Great, while in the midst of a war with Sweden, a fortress in the Russian dominions, but on the frontier of China, was defended for two or three years by a few hundred Cossacks against seventy thousand Chinese. When it capitulated, the latter were so struck with admiration at the defense that they spared the lives of the garrison, on condition that they should become a bodyguard to the Emperor. To this they consented, on the condition that they might have sent from Russia a mission of the Greek Church. The mission thus established has continued down to the present time. Every year is sent overland to Pekin a deputation from the Greek Church in Russia. They have an astronomer attached to the Embassy. Two of these have been educated at Poulkova. One of them, when there, drew up a list of astronomical works transferred to the mission from the mission of the Jesuits when they were expelled. In this list M. Struve found many rare works, and the Minister of Public Instruction, on hearing this, ordered them to be transferred to Poulkova, where they now are.

. . . .

Poulkova, August 21, 1851.

In the evening we observed in the dome till 2 A.M., when it clouded suddenly. I cannot decide even now upon the relative capabilities of our telescopes without a very fine night. The difference is certainly not great. My impression would be rather in favor of the Poulkova. The purple seems not so evident in it. When the image is out of focus, the image is oval.

Berlin, August 28, 1851.

After dinner, at 4:30 P.M., I went again to Mr. Fay's. He was not at home. Then to the observatory before dusk. I met M. ENCKE and M. GALLE. I went over the observatory, and then had about an hour's talk with Professor ENCKE about his method of computing an orbit of a planet, which he is about to publish in the next *Jahrbuch*. He does not believe much either in the new ring of *Saturn*, or in the new method of observing by the electro-magnetic telegraph. They will work their own way.

BERLIN, August 29, 1851.

I took the daguerreotypes of the moon and Professor AGASSIZ's letter and memoir, and sent my letter to M. ALEXANDER VON HUMBOLDT. We were introduced to the presence of the great philosopher. The first subject was the eclipse. He showed to me the various diagrams sent to him, which agree in the main features with what I have already seen. He is deeply interested in the subject. He showed me the daguerreotype of the eclipse and the corona.* It was, however, on a very small scale. The corona extended 8′, and was unequal in different parts of its circumference. M. ENCKE had spoken to me of it, and it is regarded as of great value. But it suffered in comparison with our plate, which I afterwards presented to M. VON HUMBOLDT. He was greatly delighted with it. I saw some beautiful specimens of photographs upon paper.

* Probably the one by BUSCH of Königsberg.

Leipsic, September 2, 1851.

I went next to Doctor D'Arrest's. Such a strange, old, out-of-way place as I found him in! In looking up his room I roused a couple of the queerest beings—students of theology and philosophy, or something of the kind. I found him after some little trouble—a youngish man of thirty or more,* unassuming, with a remarkable head, both metaphorically and physically. After leaving him, having seen the instruments which are mounted one hundred feet high—a good equatorial of six-inch aperture—I had just time to reach my hotel and leave for the train to Halle. Doctor D'Arrest kindly accompanied me there and to the residence of Doctor Rosenberger. The latter is a fanatical royalist, and, notwithstanding his fame, says of himself that he is not fitted for astronomy. He may be fifty years old.†

Returned to the station to a smoke and beer until the time for the train, when I parted from Doctor D'Arrest, a true German man of science! How much he loves astronomy for its own sake may be inferred from his doing what he does for a salary of forty pounds. Encke receives but thirteen hundred thalers, Gauss but one thousand thalers. Doctor Gould, he told me, was probably to take Doctor Goldschmidt's place as Second

* D'Arrest was born 1822.

† Born 1800.

Professor of Astronomy at Göttingen, GAUSS being the first.

GOTHA to CASSEL. Wednesday, September 3, 1851.

At 9 A.M. I went about a mile outside of the town to see Professor HANSEN.* A pleasant old gentleman, as almost all the great men are. He showed me his manuscript of the new lunar tables as far as completed. They will be completed in two or three years; but he will publish an ephemeris for 1852. There are one hundred and forty inequalities from the sun, and a new one of 1″.3 from *Mars*. He thinks it possible that the magnetic influence of the earth may affect the moon's motion — a reflex action of the effect produced by the moon on the earth's magnetism. The arguments of the tables are in days, from which a great advantage is derived in computing a number of places at equal intervals. Each argument is carried to $0^s.003$ of time, when necessary to give the inequalities to 0″.01 of arc, so that the sun shall be exact to 0″.1 of arc. He expects the errors of the tables not to exceed 1″.0, one second of arc. Observations used since 1750, down to 1839, at which period he has STRUVE's Dorpat observations. The tables will not be ready for the press before the above-mentioned time. This form has some resemblance to that of CARLINI's tables of the sun. Professor HANSEN's

* Born December 8, 1795.

tables of the sun, he told me, are now printing at Copenhagen, by the Royal Society of Sciences there. I went into the little observatory, and saw a little contrivance for using only seventy-two [divisions] upon the circle (for every 5°), and another arc of 5° used differentially, so that there can the more easily be formed a table of corrections of this arc of 5° for the error of division.

GÖTTINGEN, September 4, 1851.

At ten I went to the observatory to see Professor GAUSS. It is a little singular that the landlord should not have known where he lived. He knew Doctor GOLDSCHMIDT, or rather had known him.* GAUSS had just left the house, to be at home at eleven. I walked half a mile or more into the country to spend the time, returning at eleven. He had not come back. I returned to my room, and presently after some one knocked at my door, and in walked Doctor GOULD, who, singularly enough, had arrived from Altona at two in the morning, and I at four. Our rooms were almost opposite to each other. He appears well, and must have improved since leaving home. At dinner there were six Americans, four students, besides Doctor GOULD and myself. After dinner I repaired once more to the observatory, and spent half an hour in conversation with GAUSS, and gave him the last daguerreotype of the

* Died February, 1851, *æt.* 44.

moon I had left. It was the best of the first series. He showed me daguerreotypes of his son and grandson now living in America—in St. Louis; he has another son there also. He showed me his library, which can scarcely contain above 700 or 1000 volumes. He had COOPER's works; Merry Mount, a history of the country about Boston before its settlement, published in 1849; also FROTHINGHAM's Siege of Boston, with which he seemed much taken. He brought out also Doctor BOWDITCH's translation of the *Mécanique Céleste.*

Shortly before leaving he spoke of Mr. PEIRCE's position with respect to what he said about the discovery [of *Neptune*] being accidental. He thought that the calculations of both ADAMS and LEVERRIER rested on an "infirm" basis, inasmuch as the assumed distances were so wide of the truth. That the discovery was accidental, and might have failed because the planet could have been 30° from the predicted place.

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Staid up in Doctor GOULD's room until near one o'clock in the morning, talking of various subjects. He had just seen GAUSS, who had intimated to him the probability of his taking his professorship after his death. This is certainly no small honor, offered as it is to an American, and by such a man as GAUSS, while there are so many in Germany who would be glad of the situation, D'ARREST among others, whom Doctor GOULD mentioned as equally fitted for it.

GENEVA, September 14, 1851.

. . . .

I think I must go to Chamounix, to try whether it may be possible to discern the red flames on the sun's disc, by occulting all but the very edge upon one of the lofty peaks. It seems to me not altogether impossible. Certainly an experiment worth trying, and a new application of the "Aiguilles."

GENEVA, Monday, September 15, 1851.

I went to see Professor PLANTAMOUR, and was never more puzzled to find my way than to Hotel de Ville 74. Found M. PLANTAMOUR a young man of thirty, and very good looking *for a savant.* I went over the observatory and saw the meridian circle by GAMBEY. The axis may be too long, otherwise a good instrument in appearance. The equatorial has circles of nearly two and a half feet; axis supported above and below the telescope, as with the English plan. Telescope of four or five feet, four-inch aperture; object glass very much injured by efflorescence, so that they are about purchasing a new one of five-inch aperture of MERZ. M. PLANTAMOUR had commenced anew calculations on BIELA's Comet, but has discontinued them, till its next appearance.

BONN, October 1, 1851.

. . . .

I went in search of Professor ARGELANDER, and in trying to find the observatory came to the Minster and stepped into it, but saw nothing to detain me more than a minute. With some trouble I at last found the observatory, which is a large and not a bad-looking building, recently erected, I should think. I met Professor ARGELANDER, and saw the two principal instruments—the transit circle now used for zones of stars to Declination 31° south, which are nearly complete, and the transit used for the northern zones. They can observe three stars in a minute at the utmost for a short time. On one paper is recorded the right ascension, by Professor A., and on another the declination, by the assistant. There are seven columns on each sheet, and the place where the times are entered thus denotes on which of the seven wires the transit was taken. The piers are cased with wood, to prevent their being affected by changes of temperature from the body. This alone shows that Professor A. belongs to the Poulkova school. Instruments by PISTOR and MARTINS of Berlin—I did not like them so well as REPSOLD'S. I saw Mr. SCHMIDT, the assistant. Professor ARGELANDER has no confidence in electro-magnetism applied to observing. Mr. S. praised very highly the daguerreotype of the moon taken at Königsberg at the eclipse, a copy of the original which I saw at Berlin.

LONDON to CAMBRIDGE. October 16, 1851.

I started at 7¼ A.M. for the Shoreditch terminus, to take the train for Cambridge, as by appointment with Professor CHALLIS. . . .

The *Times* I had bought at the station contained the agreeable intelligence that a Council-medal had been awarded to the spring-governor chronograph at the great exhibition, and a prize-medal to Mr. WHIPPLE for the daguerreotype of the moon. . . .

I found Professor CHALLIS at home, and at his lady's suggestion and invitation, I took up my abode at his house. From eleven to near two I was with Professor CHALLIS examining the instruments. A very nice method is made use of by him for observing the form of the pivots of the transit, applicable to all similar instruments. A brass end piece may be attached to the extremities of either pivot, in which a small, finely marked dot is made, and as the transit rotates, the co-ordinates of this dot are observed by a micrometer microscope.*

There are two assistants, but the reductions can with difficulty be brought up with the observations,

*This method is now used in the meridian circles of Strasburg, Mt. Hamilton, etc.

so that Professor CHALLIS is half inclined to give up continuous meridional observations for awhile until the printing is in a good stage of progress. Then to the great equatorial. The method of adjusting the lenses of the object glass upon each other is entirely successful. The dome is a good example of lightness — of thin zinc on iron braces.

In the afternoon I walked into the town to the lodge of the Master of Trinity. Met there Professor MILLER and Professor LISTING of Göttingen, and then went to the great hall where the college dine. At the head of the table Doctor WHEWELL, on his right Lord CAVENDISH, next the Vice-Master, Professor SEDGWICK, myself, Professor LISTING, Professor MILLER. On the left of Doctor WHEWELL, Lord FREDERIC GOWER, son of the Duke of Sutherland, and Lord ———, etc. At the next table the Masters of Arts, the Bachelors of Arts, descending to undergraduates. The hall is a fine specimen of the olden times, with its noble oak roof and ceiling. After dinner the upper ten retired to the apartments of the Master, there remaining for an hour with conversation, dessert, etc. Then we inspected the portraits of NEWTON and other distinguished men, belonging to the fine suite of rooms assigned to the Master. Later in the evening I went again to the observatory. A large party there. Among them the Provost of Eton College and one or two university dignitaries. Three noblemen—Doctor WHEWELL, Mr. ADAMS, etc., assembled for a look through the great telescope, the scene a perfect counterpart of what we have at home on similar occasions. I had no fair opportunity of seeing through the telescope, as the atmosphere was decidedly bad, and on that account retired at eleven, after a good opportunity of conversing with Mr. ADAMS. He has recently detected errors in BURCKHARDT'S tables of the moon's parallax, amounting to 5″, both plus and minus. He was led to the detection by the article of HENDERSON'S on the lunar parallax, by an unaccountable discrepancy between results which should have been identical, derived from BURCKHARDT'S and from DAMOISEAU'S tables. The former, after applying Mr. ADAMS'S corrections, agrees with the latter, the mean correction being 1″.3, which is precisely the mean discrepancy. This extraordinary defect of these tables affects all the parallaxes of the *Nautical Almanac*, etc., of the Berlin *Ephemeris*. In PLANA'S theory is a term of 2″, with an erroneous sign. In LINDENAU'S of *Venus*, the secular terms are so entirely wrong that the calculated term may be double its true value, from its being obtained from too short an interval.

CAMBRIDGE, October 17, 1851.

After breakfast at 9 A.M., at which Mr. ADAMS was present, I went to the dome and saw *Arcturus*, the atmosphere being very tranquil. The appearance of a star in it is quite of a different character from that in the Munich glasses, which have a family likeness. The pink on one side of the focus, and the green on the other, are of a much purer, and more decided hue, than in the latter, yet I cannot say that the definition was inferior. The disk is smallest between the green and purple, and very nicely centered, which must be ascribed to the means possessed in the Northumberland equatorial for bringing the lenses to very exact relative adjustment. The brightness of the pink and green seems perhaps to indicate that the dispersive powers for the different colors are not similarly proportioned in the crown and flint lenses to those of the material used for the Munich lenses. The going of the clocks is much better than ours.

About 1 P.M. left with Mr. ADAMS for the town. On our way we had a long conversation about *Neptune.* He gives it decidedly as his belief, that the direction was the element most likely to be correctly given, and that it would be possible to show, independently of trial, in other words by analysis, that the change of direction of the major semi-axis on different hypotheses would be smaller than the corresponding changes in eccentricity, etc. I understood him to say that he was impressed with this before the calculations were completed, and that a first trial, with simply a circular orbit, gave a place differing only 10° from subsequent results, and that the errors in the other elements would always tend to cancel each other and give, comparatively, a true direction.

Secondly — That there is no assumed distance to be found between the true place of *Neptune* and that calculated by LEVERRIER and ADAMS, where the observations would not be better satisfied the nearer the truth was approached. That LEVERRIER's limits are vitiated, from the fact that terms, of which, from three hypotheses, no knowledge could be gained, and were of necessity omitted, were actually of very sensible amount.

Thirdly — That the reason LEVERRIER has since given why his limits failed — namely, that a wider range of error ought to have been assigned to the earliest observations, is set aside by Professor PEIRCE's showing that *Neptune* satisfies them very exactly.

Fourthly—That neglected terms in the perturbations of *Neptune* on *Uranus* were very sensible in amount, and that LEVERRIER's limits were unwarrantable on that account.

We went to the library of Trinity College, a fine room, and saw the statue of NEWTON by ROUBILIAC, also the statue of BYRON by THORWALDSEN — the most exquisitely beautiful profile

I ever saw, but it is said not to be faithfully like BYRON. I saw also the cast from the face of NEWTON. In most of the busts and pictures the lips are compressed, but ROUBILIAC represents the mouth just parted. Last night I remarked, spontaneously, a resemblance between the features of Mr. ADAMS and NEWTON'S. The nose is very like, and the mouth much the same. I was a little struck afterwards at hearing that others remark the same. I dined in Mr. A's room with him, Professor CHALLIS, Mrs. CHALLIS and their son. Mr. A. is Proctor, and among his insignia of office is a sort of pole-ax, and a standard measure much the worse for age and wear. Previous to dinner Mr. A. had taken me to King's College Chapel, through the noble interior, more beautiful and grand than I can describe, upon the stone roof, and yet higher on the wooden roof above all, in a state of preservation no one would think possible after four centuries. I left just in time for the last omnibus, having spent the day no less agreeably than profitably. Mr. ADAMS, indeed, has given the whole of it to me.

IRELAND, October 24, 1851.

Arrived at DOOLEY'S Hotel (inn) about sunset, and, as the evening was clear, I went almost immediately to the castle (Parsonstown). I met Lord ROSSE, who very kindly invited me to stay at the castle. I saw first the telescopes, and after dinner found the younger Mr. STONEY looking at *Saturn* with the three-foot speculum. The speculum had just been polished and placed in the tube a day or two previously, before it was quite complete, as the figure, they said, is slightly elliptical (in order to give Mr. LAWRENCE an opportunity of seeing the moon, which, however, clouds entirely prevented). The evening was pretty clear, and not unfavorable, though the images were in too much commotion for a trial [*i. e.* test] of the telescope. However, the two inner satellites were seen without the least difficulty, as well as the division of the ring and its shadow on the ball. I saw no distortion, wings of light, nor defect of any kind that might not have resulted from the ordinary atmospheric disturbances, with so large a reflecting area. This characterizes the nature of the imperfections of definition, which, though better than I had expected to find, was certainly inferior to that of the large refractors on nights below the average. Yet I think I have seen states of atmosphere when our telescope has defined much worse. We then went to the six-foot and saw the nebula in *Andromeda*, and I spent sufficient time on this object, on which I am at home, as it were, to acquire an adequate conception of the amount of light collected; but this, as it is a necessary consequence of the aperture, it is not needful to dwell on. What was of much more importance, was to observe the very remarkable increase in the number of very small stars close to the nucleus, convincing as to the superiority in resolving power of this telescope over ours, on which before I was doubtful, as I thought that our better definition would more than compensate for loss of light. But a long and careful examination of the region of the nucleus convinced me to the contrary. Indeed I could not but admire the numbers of small stars brought in sight. The elder Mr. STONEY assured me before that the great speculum was not by any means in high condition. Nothing that I saw led me to suppose the six-foot inferior to the three-foot, though undoubtedly it is so. The difference is less at any rate than the proportionate difficulties of construction. Both of the telescopes are in use, it seems, in favorable weather, almost constantly. The younger Mr. STONEY is now taking his brother's place. Very high powers are used; the ordinary one with the six-foot is 465, and the lowest 220. But very large fields are given, and, of course, the boundaries of the field are indistinct, and would be so under any circumstances. In forming an opinion of the performance of these instruments, this must be remembered. After midnight it clouded up.

PARSONSTOWN, IRELAND, October 25, 1851.

I breakfasted 10 A.M., a very proper hour after a long day's travel and up to 2 A.M. After breakfast

I went with his lordship through the machine shops and foundry, where the specula are made. Everything bears witness to the great amount of work gone through with before attaining the results for which the place is renowned. There are piles of models and plans, from the earliest efforts up to the present moment. It is well to see that a vast amount of real work has been first gone through with, and I constantly meet with evidence of the mechanical ingenuity of Lord ROSSE in contrivances suggested by him. He was with me nearly all day, and his knowledge of mechanics reminds me of Mr. BORDEN'S. By a most fortunate coincidence, they appointed to-day for polishing a three-foot telescope. It takes six hours, and the whole process is so completely by machinery that the superintendence of a competent person occasionally to moisten the rouge and to watch the process, rather than to take part in it, is all that is necessary. Constant improvements are being applied. "Well enough" is not an admissible term. They now are on the point of trying to cast specula with honey-comb backs, to get rid of weight and flexure. A small steam engine carries the polisher, etc. The six-foot mirror, which is thickest and heaviest, has the advantage in flexure. I saw the whole process to the completion. I spent the evening with both Mr. STONEYS at the telescopes till 3 A.M., when it entirely clouded. Saw with the three-foot the new ring of *Saturn*, not well; and with the six-foot, among other objects, the cluster in *Perseus*, and was impressed with the contrast of colors of the stars brought out by the light of the instrument. The night grew very unfavorable, and before 3 A.M. clouded entirely.

LIVERPOOL (MR. LASSELL'S OBSERVATORY),
October 30, 1851.

. . . .

Later in the evening we repaired to the dome and looked at *Saturn*. The state of the atmosphere was unfavorable, but I was entirely satisfied of the excellence of the telescope, making allowance for the atmosphere. In point of definition it must be allowed as a rival of the great refractors, and in light (two feet aperture) surpassing them. The air was too bad for the severer tests of double stars. The new means used for counterpoising all parts of the mirror is thought a great improvement. On the whole, after seeing pretty thoroughly Lord ROSSE'S operations, and acquiring at least an idea of the high degree of excellence of Mr. LASSELL'S specula, I am disposed to think that, in the hands of those who, like them, apply so much attention and care to the subject, the reflectors are in a fair way to eclipse the refractors, in definition as well as light, when very large. The new ring of *Saturn* was plainly seen; its breadth we both agreed [to be] less than that of the outer ring, yet occupying nearly half the distance between the ball and the inner edge of the bright ring. The dark space, including the new ring, is less than the breadth of the old ring. We both agreed on this.* We looked at *Uranus* and saw four, perhaps five, satellites. I think the greater light tells on the performance here.

EXTRACTS FROM THE DIARY OF 1863.†

Left Boston 11 A. M., April 15, 1863, by steamship *Canada*, Captain Grace. A light head wind to Halifax—4 A. M., Friday, April 17th.

AT SEA, April 19th.

A light southeast wind. We are sailing at eleven and one half knots. To-day we met the *Persia*, bound for New York. She signaled having seen ice in longitude 43°.

Iron steamers observe azimuths of the sun with the compass twice a day. The captain considers the lighthouse system of Massachusetts Bay excellent. He has heard pilots complain of the new dioptric light as not being so good as the old one

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*Modern measures confirm these early, but careful, estimates.

†The journey of 1863 was undertaken for the purpose of determining the cost of an objective of large size for the Harvard College Observatory.

in fog and snow. It does not throw the light *up* on the fog above it, so as to show its position. The green light carried by steamers is seen from one to two miles farther than the red.

AT SEA, April 20th.

A warm southerly wind. Seven icebergs are in sight at once. We saw twenty or more last evening. The course was altered to avoid two of them, which we passed within one eighth of a mile. Those on the leeward side were brilliantly white in the sun, the spray dashing over them changing their aspect remarkably as they passed. One of them at first seemed a mere square block, or table, thirty or forty feet above water,—as it passed it changed to a mass of turreted peaks. Another was very beautiful, with sides jutting over the water in a curve—mushroom-like.

AT SEA, April 23d.

We are just half way over, and to-day I saw a petrel, but no gulls or other birds. Very fine weather and a fair wind—the ship making her best. It has been, with the exception of the 21st, good weather since losing sight of ice.

April 24th.

English sea birds were first seen this morning. They do not fly so heavily as our common gull. The wings are tipped with black.

About four years ago the *Canada*, in the daytime, going eleven or twelve knots in a dense fog, struck full against an iceberg. The bowsprit touched first, and was carried away; then the figurehead and all the light work was destroyed; but, fortunately, her way was stopped before any damage was done below water line. The engines were reversed a moment before and diminished the speed about one knot.

April 26th.

Arrived at Liverpool, Sunday, 8 P. M.

LIVERPOOL, April 27th.

I went to-day to the observatory and found Mr. HARTNUP. His son, JOHN HARTNUP, is now attached to the observatory. All the arrangements seemed very neat, but Mr. H. complains that little time is left for astronomy after attending to 200 chronometers and the Meteorological Record. The self-registering wind curves are very effective. I saw evident spots of efflorescence on the object glass (by MERZ) of the equatorial, just as on ours. Mr. HARTNUP has no difficulty in getting all necessary provisions for ordinary expenses. He thinks $1000 small for our observatory.

BIRMINGHAM, April 28th.

I arrived at Birmingham at 12:30, and at 1:45 returned on the line as far as "Spon Lane," where CHANCE Bros. & Co. have their great glass works. I saw Mr. JOHN CHANCE, and he gave me the following information:—

They have a department specially devoted to optical glass, of which the manufacture is a secret. The demand for photographic camera lenses is very great. They made two discs of 29-inch, which I saw in the great exhibition of 1851, and they have on hand two discs (one crown and a flint to match)* of 25-inch diameter, and a flint of 20-inch. The latter, with a crown to match it, would cost in its present state £600 for the pair, and one inch might add £100 to the cost, but could not be made to order under six months' notice. The following are the dimensions of those discs which I saw and examined carefully:—

(A)†—Disc of crown: diameter $25\frac{1}{4}$ inches full, $2\frac{3}{8}$ inches thick.

(B)†—Disc of flint: diameter $25\frac{1}{4}=1\frac{13}{16}$.

(C)—Disc of flint: diameter $20\frac{1}{8}$ to $20\frac{1}{4}=1\frac{3}{4}$.

(A), density 2.50; (B) and (C), 3.60.

(A) weighs about 100 pounds, (B) about 140 pounds.

(A) was [put in place for examination], and as the edge had been ground and polished with opposite parallel faces on the edge (as were also the

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*An object glass is usually composed of two lenses—one double-convex lens of crown glass, one plano-concave of flint glass.

†These are probably the discs used in 1870 for Mr. NEWALL'S 25-inch refractor, now at Cambridge, England. It is understood that the price of the rough glass was £1000, and the total cost of the instrument mounted, complete, £3950.

others), by looking through, with light from the window opposite, there was good chance for detecting striæ. It appeared to me that this disc was of high quality, for, after thorough exploration, I could detect none of any moment, indeed I might say none at all, for those noticed were mere faint wisps and very small, attached to the usual specks or bubbles.

Next we looked at (B). This is less transparent than (A), and I think inferior to it, though Messrs. CHANCE have a high opinion of both, and consider them of better quality than that furnished by Mr. CLARK. The flint, they say, gives far more difficulty than crown, and to make to order discs of large size, say exceeding twelve inches, they need from six months to one or two years' notice. The difficulty is, that to make large optical glass, even for a single lens, they must interrupt their other business, and cannot fill orders in their regular branches of trade. This they more than once alluded to as a serious matter. There is not demand to warrant them in undertaking the manufacture on a large scale, but if not limited as to price, for £10,000 to £50,000, they said they could make discs of thirty inches.

(C), I thought, had a decided defect, but it was impossible to be assured of the real optical value of either disc by such trial as could be made.

The Messrs. Chance appeared very frank, and as to the test afforded by mere inspection, I should be disposed to rely on their judgment. They deal largely with Voigtländer for cameras. Plates of one half inch thick by twelve inches they sell in large quantities, to be cut up by opticians. They have discs of eight inches, which are unsalable, simply because the glass is not of the usual tint. One that I saw was violet.

There are now at least two parties in treaty for the 25-inch disc, and they mentioned Mr. Clark as having lately made inquiries for a disc of twenty inches. Mr. Newall, an engineer of wealth, will probably purchase the 25-inch, and they expect Cooke of York, soon, to examine them. They would scarcely undertake to make a 20-inch to order at less than six months' or one years' notice.

The general establishment of the Messrs. Chance is immense. Glasshouses cover twenty-six acres, and alkali works some eight more. In the glass-works they employ 1600 workmen, and in the alkali 600. Total, 2200. They are changing their furnaces to melt by gas, instead of by coal. This is creating a revolution in glass making, as the color of the glass is improved. They make the "French lens" for light-houses—in fact, the whole dome of the light, revolving machinery, etc.—and they have for this an establishment and machine shops like a locomotive factory.

LONDON, April 29th.

Left Birmingham this noon. The country between there and London is indescribably beautiful.

I tried to recognize what the peculiarities in the landscape were which make it so different from ours. I think one is, that not a square rod of ground has been left in a state of nature. The fields are all smooth and garden-like as far as the eye can reach. The hazy atmosphere has something to do with the effect. The color of the building materials is also quite different; no bright tints —all dingy red, or clay or stone color. The soil is mostly reddish, which I have noticed brings out the green of the grass and the complementary colors. The style of building is also quite different. We have no such country bumpkins as the laborers in the fields.

LONDON, May 1st.

I drove to the Crystal Palace to-day. The building is a marvel as to size and imposing effect; the grounds, too, are pleasant, but the contents of the building and the display of articles are altogether inferior, and not to be mentioned in comparison with the Exposition of 1851.

LONDON, May 3d.

I went to-day to the service in Westminster Abbey. There was a large congregation, and being late I was too far outside to hear perfectly. The chanting of the responses was beyond description impressive, filling the temple with a mighty tide of heavenly sound. One does not need to distinguish perfectly the words uttered, in order to bow the head and worship there. I love the old abbey better every time I look on it.

LONDON, May 5th.

I went this morning to Palace Garden Terrace for Professor MAXWELL, and found him at home. At the Royal Society, Saturday evening, I saw his apparatus to illustrate the motions of a ring of thirty-six satellites about *Saturn*. He does not think the constitution of satellites conforms with the aspect of the ring. He has discussed the subject of the ring being a disintegrated solid. He states that the loss of force by friction and heat would not be appreciable to observation, supposing there were perpetual collisions. So loss by friction of a fluid would be inappreciable. He doubts if a ring of satellites would satisfy the observed aspect.

He referred to the aspect of the moon at full having the rim brightest, as probably an indication of a rough surface of large blocks—not fine sand.

I saw to-day Mr. JAMES and WM. SIMMS. The latter showed me object glasses of eight inches in process of construction. He thinks the bubbles of no consequence. Merz flint is quickly affected by the English climate. CHANCE says the bubbles come from stirring when the glass is melted, and, when not stirred, striæ will show themselves. MERZ manufactures his own glass. I saw Colonel STRANGE, who had come to look at a casting just from the mold, of an aluminum bronze circle for a theodolite for the India survey. It is the largest casting of the bronze yet executed, and proved to be a very good one, though Mr. SIMMS had just told me that the metal was difficult to cast, being viscid. The color was very fine, like gold, but tarnishes if handled. The bronze is exceedingly tenacious. It costs five times as much as bell metal. Its lightness, rigidity and small expansibility are recommendations for a large meridian circle. In the afternoon I went to Crawford, where I met Mr. DE LA RUE. The country was surpassingly beautiful, and the season one of the earliest on record. Crawford is a fine place. On the way from the railroad station we passed over Hounslow Heath, now a lovely garden. We drove through a noble avenue of oaks, and such lawns, shrubbery and walks! like fairy land.

I saw the photographic process. Mr. DE LA RUE uses cadmian collodion. He has an exhausted air plate holder; the plates are small, circular, two and one half inches. His telescope is a reflector, thirteen inches aperture, ten feet focus. He has now a STEIN-

HEIL mirror, not yet tested. He thinks a reflector has advantages as to curvature of the image, besides, there is less absorption of actinic energy. OTTO STRUVE was here two weeks ago. He has very strong opposition to contend against. The Academy alleged the cause that he is a German, and there is an intense manifestation of nationality in Russia just now. DE LA RUE's stereoscopic pictures of the moon in various phases have the distortion of looking ellipsoidal, the longer axis to the eye, or a little below it, when seen as in the telescope. The photographs are certainly very fine, and bear magnifying exceedingly well. He attributes it to the use of the cadmian collodion, and in not using silver in developing,—only pyrogallic acid. His drawings of *Saturn*, *Mars* and comets are very numerous, and excellently well done, evidently with great fidelity; note a stereoscope of *Saturn*—a striking thing. The shadows of *Saturn's* ball on the ring are singularly like ours.

LONDON, May 7th.

I went this morning to St. James' Hall, the dining club of the Royal Society, and there met General SABINE, Professor MILLER of King's College; Professor TYNDALL, Professor SHARPIE, Doctor ROGET and others. After dinner I went to the rooms of thè Royal Society to the regular meeting. General SABINE was in the chair; Professor SHARPIE on his right, Professor STOKES on his left. I met there Professor MAXWELL and THOS. GRAHAM, Master of the Mint, and saw also Professor HURST (tall and thin), and Professor SYLVESTER (stout and jolly).

LONDON, May 8th.

This morning I went with General SABINE to Richmond, through the deer park, to Kew Observatory. The whole magnetic apparatus is now excellently arranged, very compact and effective. It can all be comprised easily in a small observing building of the cheapest construction, about twenty-five feet square. . . .

Thence I went to Kew Gardens with General SABINE by the Deodor avenue. Everything is extremely beautiful. It is impossible to conceive of a garden more complete and beautiful. In the museum I noticed a half-length crayon of Doctor GRAY and Doctor TORREY, taken when young. In the great palm house many of the trees have been topped or cut down for want of room. There is a new conservatory of half-hardy plants of temperate climates.

I returned to London as far as Vauxhall with SABINE, who asked me to dinner, but I was already engaged for the Royal Astronomical Society Club at Freemasons' Tavern. There I saw Mr. DUNKIN, Mr. AIRY, Mr. WHITBREAD, M. P., Colonel STRANGE and Sir CHARLES BRIGHT, who invited me to see the cable proposed for the deep sea, five times heavier than that used hitherto. The telegraph now extends to Bagdad. Mr. CARRINGTON, Admiral MANNERS, EDWIN CLARKE, C. E., R. HODGSON, PRITCHARD, SELWYN, VIGNOLLES, C. E. WALKER, HIND, Doctor LEE and Mr. BUCKINGHAM were present. The last named has an achromatic worked by WRAY from discs rejected by the French Government. It was exhibited in the exhibition of 1862. One small part is defective. It has a crown of twenty-nine inches, but the flint is not good.

At the club dinner Mr. AIRY was in the chair, Bishop COLENSO at his right. He alluded to Professor SOPHOCLES's work on the modern Greek as evincing great learning, and was generally very complimentary to American science. It was a full meeting, about sixty or seventy present.

GREENWICH, May 12th.

This morning I went to Greenwich, and met Mr. AIRY at the station on his way to London. I spoke to him of Professor SOPHOCLES's work on modern Greek, and told him it must be that published by the American Academy. He assented, and called it a very remarkable work.

At Greenwich I was shown by Mr. STONE, the first assistant, and Mr. CARPENTER over the principal part of the buildings—the great equatorial—(the framework enormous) the dome a light, flat-roofed cylinder. The best form of all is, I think, De La Rue's—a simple, square, flat-roofed building. The object glass was very much dimmed by [atmospheric] action, apparently on the flint lens. There was a general cloudiness, and undoubted spots of efflorescence, like those on our great object glass, but ours is in a far better condition.

I examined the great transit circle. The illumination is very good; no trouble here with quicksilver tremor, but much was experienced with the reflex zenith tube, which is now obviated by using wooden boxes or frames suspended one inside the other, three of them, by strips of India rubber $8 \times 1\frac{1}{2} \times \frac{1}{8}$ inches. The result is perfect.

The transit circle is, I fear, in danger of having too much to do with all the galvanic connections. I saw the register apparatus. They rule the sheets in advance by ink from a pen like ours, and then use the punctures; but the record is obviously far less regular than the spring-governor. Some of the dots are very faint, and must be quite troublesome to read off.

The manuscript room is fireproof, with slate shelves, a capital thing. Here are collected all the manuscripts of observations and computations from the founding of the observatory, all bound, labeled and in perfect order. The volumes of Mr. Airy's correspondence are appalling. One set of recent date contains a letter of the Astronomer Royal, in which he states the items of his correspondence on the subject of "Sales of Gas Commissions Act" to have amounted to 431 notes, letters, or longer documents, among them long letters and one report of thirty pages. He writes all these *himself*, and has no private secretary. Nearly his whole time is necessarily consumed in correspondence and in the direction of affairs. Matters outside the province of the observatory or of astronomy consume much time.

Bonn, May 17th.

I called on Professor Argelander at 9 A. M. At the observatory I saw the principal instruments. The meridian circle is by Pistor and Martins, Berlin. He uses now only one circle, but would use two for fundamental work. The pivots are only one inch in diameter, and the level, which is a hanging one, rests on parts [of the pivots], say an inch distant from the *y*. When the level is set on, it is swung pretty forcibly backward and forward to give it a firm bearing. The microscopes are supported on brass arms. Professor Argelander doubts whether they are better than in stone, as in streams of air of different temperatures they are liable to distortions different from the circle. He thinks the idea of covering the piers with cloth a good one. He does not like gas, as it has injured the circle divisions by tarnishing. I saw the same thing at

Greenwich, where they complained of it also. The building and the outside grounds are in excellent order; within, indications are that appearances are not considered of much account. Professor ARGELANDER spoke of the desirability of making a comprehensive star-catalogue to include all scattered positions of stars in the *Astronomische Nachrichten* and elsewhere. He mentioned that HOEK of Leyden is engaged on a reference catalogue, citing places where stars are to be found. BESSEL was very fond of hunting, and used to go into the country on Saturday nights to hunt on Sundays. He worked very hard, but went often into society. He was not a good teacher, and used to tell ARGELANDER not to attend his lectures,—that he could use his time to better advantage. His habit was, in whatever subject he was engaged upon, to study up specially on that alone, and he had little idea of teaching the general principles simply.

At social entertainments he used to have a little table set for himself, in case of his leaving to observe. He was lively and very amiable. He lectured eight hours a week at Königsberg. D'ARREST is subject to melancholy. SCHWERD, at Speyer, near Mannheim, is ingenious in mechanical inventions. He is getting old, and has affection or dizziness of the head, preventing observing. KAISER is getting into years, and is subject to headaches.

SCHÖNFELD, FÖRSTER and WINNECKE are among the most promising of the young men.

REPSOLD of Hamburg and PISTOR and MARTINS of Berlin are the best instrument makers. ARGELANDER likes SCHWERD'S photometer much better than STEINHEIL'S, which is complicated. He thinks he would prefer smaller, rather than larger, circles to his meridian circle. They are about three feet. He has a fine instrument for extra meridional observations of distances of stars apart. He proposes in this way to determine R. A. differences of a few stars, independent of time, but flexure is a great difficulty. The level tube is freely exposed to the air. I saw the comet-seeker of "*Sternverzeichniss*"—a very plain, unpretending instrument. One observer could not continue the observations much beyond an hour and twenty minutes, from fatigue. They have observed as many as thirty stars in one minute and 1200 in an hour.

MANNHEIM, May 18th.

I called this evening on Professor SCHÖNFELD, and had two hours' conversation with him. He showed me his "*Nebelflecke*," two copies of which he has sent to H. C. Observatory, through booksellers. The Government did not allow him sufficient means to publish his observations in full. Professor ARGELANDER also complained that for want of means his charts were not as well printed as he desired.

I understood him to say that he furnished the money himself. He has a certain amount allowed yearly for the observatory, and although it is in form appropriated to different objects, yet what he saves in one department he can spend in another.

SPEYER, May 19th.

I went to Speyer to-day at 9:25 to see Professor SCHWERD and the photometer. Such extraordinary old battered doorways or gates to the private houses, as if they were intended to stand a siege of a week or more! Generally in German towns people seem to walk in the middle of the streets, in preference to the sidewalks, when there are any. The streets are clean, except the gutters, which perform the office of sewers. Professor SCHWERD speaks not a word of English. I saw first in his garden a little box 4x4x8 feet high, his photometric observatory! and a little octagonal building ten or twelve feet in diameter, where is mounted the meridian circle by REICHENBACH & ERTEL, Munich (the elder ERTEL, the younger has lately died). SCHWERD and ARGELANDER both thought the recent work of the establishment inferior. With this instrument the polar catalogue was observed. It reads by *verniers* to 4″, but much closer by estimation. I noticed that SCHWERD had altered the level *y's* (hanging level) so that they rested on the part of the pivot over the point of contact of the pivot on the *y's* attached to the stone; whereas at Bonn the level rests on parts of pivots inside their points of support. He also added the counterpoise to make the pivots bear equally on each *y*. The circle is perhaps of twenty inches, and is graduated as a finder on the edge and finely on its face as silver. I do not think the counterpoise for flexure of the tube can be an advantage, as it must act irregularly at zenith. Professor SCHWERD explained his photometer at length. The instrument had been taken apart, but by drawings and looking at different parts I could understand it readily.

. . . .

SCHWERD is making two, one for Poulkôva and one for Wilna, the cost 2450 thalers—not high, I suppose, considering the amount of work, including the clockwork. The optical part was made by MERZ—the rest in his own workshop.

Speyer is one of the oldest German towns, and is mentioned by Roman authors, perhaps by CÆSAR. The cathedral has been two or three times restored, and now the interior has a modern look, being finely painted and adorned with pictures of scenes from the Old and the New Testament, well executed. The ceiling is in color and gilt. Outside, in the gardens adjoining, you come now and then on older ruins, and there are many marks of an older building about the cathedral itself.

MUNICH, May 21st.

To-day I went to MERZ's Optical Institute. I found SIEGMUND MERZ and saw MERZ senior, who is apparently about seventy years old.* They showed me various object glasses to illustrate the effect of efflorescence. Some had round spots, like those on our object glass, though not so large—others irregular streaks of the same nature, perhaps—but much more extensive than any seen on our object glass. I saw an object glass of 16-inch, French, and 6-foot focus on trial. This also showed much more action than ours. In another room I saw two finished object glasses of 14-inch, French, one of which was the companion of ours or of Poulkova, and the other the companion of that made for Lisbon under the supervision of M. OTTO STRUVE, which was considered an excellent glass. A third was of 16-inch, French. This, MERZ said, was good as to performance, but in one part it had a stria which, though he did not really consider it of importance, would prevent him from selling it, as when noticed it might injure the reputation of the establishment. I observe that on this point they seem very sensitive. I saw the flint lens of the 18-inch, French (curves for focal length 27-foot, French). MERZ considers it of very fine quality, and on careful examination

* GEORGE MERZ, born 1793. From 1818 he was an assistant to FRAUENHOFER (who died in 1826), and from that time onwards he was the head of the Munich Optical Institute.

I found it remarkably transparent and nearly white in color. It is unusually free from bubbles of much size. I saw the crown glass partly ground, but this had the surface marked with one or two pretty deep seams, and it is doubtful if they would come out in the grinding. In another room were two or three discs of crown of 18-inch diameter of different colors, some bluish. These had large notches, two inches long, scooped out of them, often to the depth of one half the disc, probably for the removal of striæ. There was a large block of crown, 10x12 inches, of irregular shape, considered of fine quality, which was to be *pressed*, in a semifluid state, into the shape of a disc. This is an important part of the process. Flint heats more readily, and becomes of the right consistency for pressing, while crown glass is more refractory in this part of the process, and sometimes, by repeated heating, becomes brittle, so that for very large discs the crown is more difficult to make than the flint, although, perhaps, easier in the rough mass. I saw two or three 15-inch discs in the rough, some of which had been examined.

Merz now undertakes lenses of moderate size, under 10-inch, with ratio $\frac{\text{aperture}}{\text{focus}} = \frac{1}{12}$. He has made, or is making, one for Hamburg—an equatorial.

This afternoon I drove out to Bogenhausen and found Doctor Lamont at home, in the workshop of the observatory in the midst of work. He showed me first a new transit, not yet finished, designed for zones.

. . . .

I saw an old two-foot circle by REICHENBACH, fitted up to be used for latitude observations in a new verification of geographical positions for the meassurement of degrees.

Doctor LAMONT has a meridian circle of REICHENBACH'S—his first large one—made about 1818. It is in very good keeping, though Doctor LAMONT fills the observatory and covers all his instruments with ingenious contrivances of his own—this among the rest. I saw the nadir reflection of wires excellently. The mercury is only about 0.04 inch deep, and is in a copper or brass disc, to which it adheres slightly. Doctor LAMONT is confident that 0.02 inch is deep enough to insure horizontality. It comes to rest in twenty or thirty seconds.

. . . .

Doctor LAMONT is unmarried, and works very hard evidently. He is much of a mechanician, and works himself in the workshop at the observatory. He lives rather secluded from society, and is *un peu angulaire* in his ways. But he evidently has great resources of mechanical invention and theoretical knowledge. Astronomical observations, as well as those of magnetism and meteorology, were evidently in progress, but it was equally evident that the amount of labor undertaken must be beyond the possibility of *accomplishment* without a large staff of observers.

S. MERZ has the distinct impression that the manufacture of the large discs is a profound secret resting with the elder MERZ and SIEGMUND. But STEINHEIL, who uses CHANCE'S glass, says it is better than MERZ'S; that the latter follows the old rule to the letter, and compasses no new or original conceptions for improving upon FRAUNHOFER'S method.

I saw MERZ to-day. He is prepared to undertake object glasses of eighteen inches to twenty-one inches, French measure, with ratio of aperture to focal length $\frac{1}{14}$. He thinks that a provision for tilting, or otherwise altering the relative positions of lenses, is inexpedient, a good deal of trouble, and not much use. The time for making a large lens—say eighteen inches, French, would not be less than a year and a half, and probably two or three. The increase of weight for a lens of eighteen inches aperture, 21-foot focus, Paris, over one of fourteen inches and twenty-one feet, with a cell in both cases, would be twenty pounds. He can be quite sure of making a large disc if sufficient time be allowed. Here, as in other points, is an indication of his following a method similar to CHANCE'S.

The thickness of a crown lens for aperture eighteen inches, focus twenty-one feet, Paris, would be determined by the accidental quality of the glass. It is not likely, however, to differ outside the limits — 1.5 inches to 2.0 inches.

The flint lens need not be quite so thick — viz. about one third inch thick in the center, and perhaps one and one half inches at the edge. Together the lenses would be scarcely two inches thick at thickest.

There seems to be no objection to mounting the tube in [our] old mounting; but Merz appears to think more alteration necessary in the counterpoises than we do. The bedplate is not solid for the entire length under the present tube, and might be adapted to a new one.

Merz has not as much power to adapt himself to new circumstances as Clark has.

MEMORANDA OF COST OF FURNISHING A LARGE OBJECT GLASS (G. & S. MERZ).

Munich, May 22, 1863.

Cost of object glass in its cell simply, without tube, aperture 18 inches, Paris measure. Focal length 21 feet, Paris, ... 42,000 florins = $18,000 gold.

Time required to construct it, ... $1\frac{1}{2}$ to 3 years.

Aperture 18 inches, focus 27 feet, 36,000 florins = $15,000 gold.

Tube and finder . . . . 1,075 florins = $450 additional.

Aperture 19 inches, focus 14x19 = 22 feet (about), ... 51,000 florins = $21,900 gold.

The 1 inch additional would therefore increase the price by about $3900.

MERZ, 18.5 inches, English, ... $16,000.
CLARK, about . . . . 8,000 gold.

N. B.—All the above prices are for the apparatus at Munich, and do not include transport to New York or Boston.

MUNICH, May 23d.

S. MERZ called at 9:30. Last evening he had calculated the price for an object glass eighteen inches, French, 21-foot focus = 36,000 florins = $16,000 gold. This was very close to what I had anticipated; but I was also persuaded that he would abate it decidedly to secure the work. I told him that CLARK's object glass had been offered to us for $11,400 currency at gold premium, or $7000 to $8000 gold. He said that on the basis of an *a priori* estimate their prices would be as stated, and as such it might prove too high. He proposed no reduction, and I did not ask any. I simply told him of CLARK's offer, and mentioned incidentally that I was going to see STEINHEIL. Soon after M. STEINHEIL (the son) came in to accompany me, and MERZ left.

I was greatly pleased with STEINHEIL's establishment. They had made out a list of things to show me. The first was the adjustments of the 4-inch object glass of the new construction (improved from GAUSS). One of its advantages is the scope allowed for adjustment by the interval between the lenses.

It will take two years from date of order to make an 18-inch French aperture. The new construction will cost one third more than the old.

PRICES FOR THE OLD CONSTRUCTION.

(7 Florins=15 Francs.)

Aperture.	Focus.	Florins.
9-inch	9	1,915
12 "	13	4,220
15 "	16	7,872
18 "	20	13,171

Flint of very high density is apt to effloresce.

MERZ is making an object glass for the Hamburg equatorial, 9.5-inch aperture, 9.5-foot focus $= \frac{1}{12}$. It has prisms of flint, with a very large proportion of lead, showing the line D of the spectrum triple by a single prism of 60°. KIRCHHOFF uses these prisms.

MERZ has the reputation of following, without deviation, FRAUNHOFER'S methods. He is very reticent as to his processes. STEINHEIL polishes with glass on glass and iron on glass. The grinding is by hand, the polishing by machinery. The surfaces are frequently tested.

I was struck with the frankness of the STEINHEILS, father and son, and the readiness they manifested to show me all their processes, although I had in no way intimated that I had any proposal for a large object glass, or any order in contemplation which might induce them to think it for their interest to be attentive.

All speak of STEINHEIL as a man of genius and very eminent. Besides being proficient in painting and music, he has lately given special attention to acoustics. His two sons are highly intelligent.

MUNICH, May 23d.

This evening I went to VON MARTINS' to dine with Madame VON M., Doctor BEZOLD, a *privat docent*, Professor SEIDEL and their nephew. AGASSIZ was a pupil of VON MARTINS. It is plain that VON MARTINS has a sound estimate of his position and character. He is a shrewd man. I was quite prepared for his opinion, expressed in the strongest terms, of Dr. ASA GRAY'S high standing. He has a weakness for collecting violins, and showed us a number by various makers. The peculiar form of the instrument is essential — is many centuries old — but the reason no one can tell. I returned home with Professor SEIDEL and Doctor BEZOLD.

PARIS, June 3d.

I went to-day to the observatory. The interior is in admirable condition as to appearance, fine polished oak floors, etc. I saw the principal instruments, among others the Foucault mirror of 15-inch to 20-inch, French. One is in contemplation of 46-inch, French. It rests on an air bag, which is inflated at pleasure until the figure assumed by the mirror is satisfactory. I saw, also, the two 29-inch discs by CHANCE & Co., the largest ever made, and examined them, though the light was not suitable. I was very favorably impressed with the quality of the glass. M. LEVERRIER stated that the crown lens was perfect—not a defect could be found on the closest inspection. The flint is very good, but not equal to the crown. STEINHEIL was a member of the committee who examined one of these discs in the London Exhibition in 1857, and he told me it was very good. They have no present intention of working these lenses, being occupied with the mirrors and with the establishment of an observatory in the south of France. The new transit-circle, of the size and after the style of the Greenwich, has just been mounted, and M. EICHENS is still occupied upon it. Illumination of divisions by means of prisms is excellent. Divisions cut with a steel point look very nicely cut. As with the Greenwich instrument, there is no provision made for reversal. M. LEVERRIER will not use reflection from nadir for collimation, nor determinations from collimators, "because in the nadir and the horizon the collimation error is not the same as in ordinary positions of the telescope." The instrument is of cast iron, about twelve feet long. LEVERRIER showed no liking for the electric method for ordinary observations; thinks it no more accurate than the old method, and liable to greater changes of personal equation.

The clock used for telegraphic signals has a gridiron pendulum, and the circuit is broken directly by the pendulum acting on an inclined plane, as long ago in America. In adopting the principle of the relay magnet setting off a strong by means of a weak circuit, M. Leverrier seemed, as I understood him, to be under the impression that the idea was a new one. I think the standard clock is not put on any magnetic circuit whatever. To-night it was proposed to communicate signals for differences of longitude with Strasburg and Brest, and all the preparations had been made. M. Leverrier invited me to be present, but I was convinced that this would endanger an interruption of the course of observations, and declined, subsequently sending a note to thank him for the courtesy, and explaining my motives. I know that in my case I would thank no one to disturb similar operations by even his bare presence.

CHAPTER IV

SELECTIONS FROM THE CORRESPONDENCE OF GEORGE BOND 1852-1865

IN this chapter a selection from the letters of GEORGE BOND is given. It is impossible to print anything more than a selection. Those letters have been chosen which exhibit the activity of the observatory, throw light on BOND's own researches, or serve as documents for that History of Astronomy in America which is still to be written. Letters dealing with the details of scientific work must needs be omitted in this place. They are of interest to astronomers, but they require too much elucidation to be read by non-professionals. The commentary would usually require to be as extended as the text. Occasionally BOND has explained his plans and his work in familiar letters to friends, and such letters are always printed in full. His whole correspondence contains singularly few paragraphs which do not deal with scientific matters. The only rays of humor come in stray passages in the letters of that very accomplished and learned man, Doctor PETERS, Director of the Hamilton College Observatory; and these often cannot be quoted, as they frequently relate to very personal affairs. One of BOND's most faithful correspondents in England was Mr. R. C. CARRINGTON, Secretary of the Royal Astronomical Society, 1857–62.

From 1851 onwards many letters passed between them. Nearly every letter is of interest to the professional astronomer, and for that very reason is too technical to find a place in this collection. They are full of details relating to CARRINGTON's two important works (his catalogue of polar stars and his observations of the solar spots) and of the work going on at Harvard at the same time. A number of very interesting letters exchanged with the Rev. W. R. DAWES (mostly relating to the dusky ring of *Saturn*) must be omitted here also, on account of their technicality. The case is similar with the correspondence between BOND and Prof. W. A. NORTON, of Yale College (on the theory of the internal constitution of comets).

One may say that nearly all of BOND's published work is to be found, in embryo, in his correspondence; and conversely, that there is comparatively little in his correspondence which may not be found in his printed memoirs. Astronomers, then, will lose relatively little by the rigid exclusion of his purely scientific letters, which it has been necessary to make. They are in a position to consult his scientific writings, and to follow in them the processes of his mind. The chief loss will be to nonprofessional readers who are not familiar with his published work.*

During the year 1858 BOND conducted a large correspondence with astronomers all over the world in relation to the theory of the great comet of 1858, and for the purpose of collecting all observations and measures made of the nucleus and envelopes, and all drawings of the tail. These letters of BOND's are highly interesting, as they show him in the very midst of his work, and exhibit the processes of his thinking. They are, however, entirely too special to be printed here. The correspondence of this and other years has been copied by his daughters, who have most kindly presented their copies of the original letters to and from BOND to the Lick Observatory, with the sanction of the Director of the Harvard College Observatory, Professor EDWARD C. PICKERING. The sheets have been uniformly bound in volumes and deposited in our library, where they will be carefully preserved and always available to astronomers in future years.

I have omitted the many letters of condolence written to BOND on the death of his father,† because

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* A list of his scientific writings is given in Appendix II.

† By Professors BRUENNOW, CASWELL, GIBBES, HUBBARD, MITCHEL, NORTON, PETERS and others; by President or ex-Presidents JOSIAH QUINCY, EDWARD EVERETT, JAMES WALKER, of Harvard College; by Captains WHIPPLE and

they are chiefly important as throwing light on the character of the latter; and because the biographical sketch printed in another chapter of this book gives a more complete and symmetric presentation. They, one and all, exhibit the high esteem felt for the character of the elder BOND and friendly interest in the career of his son.

PROFESSOR J. F. ENCKE TO GEORGE BOND.

(TRANSLATION.)

BERLIN, May 19, 1852.

Most Honored Sir:—

Director HANSEN writes to me on May 15th to say that you have written to him that the method of calculating the perturbations from the original equations for rectangular coördinates, which I printed as new in the November number of the *Monatsberichte* of the Berlin Academy had been already published by you on May 29, 1849, in the *Memoirs* of the American Academy. . . . I at once looked at your paper "On Some Applications of the Method of Mechanical Quadrations," and found that your method and mine are, in fact, entirely identical in form. . . . It is not possible for me in the least to contend as to the priority [of your paper]; and I therefore hastened to make a communication to the Academy [on the subject], in which your rights are fully acknowledged. On the other hand, I can assure you that I had not the slightest idea that you had discovered this method before me, otherwise I should have declared it at

GEORGE MEADE and Colonel GRAHAM of the Engineer Corps of the Army; by Captain WILKES, Lieutenants MAURY and GILLISS of the Navy, etc.

once. . . . In the present case, before I printed my memoir, I communicated my method, by letter and at length, to Hofrath GAUSS, Professor AIRY and Director HANSEN, and since it appeared to all of these gentlemen to be new, I considered that nothing similar could have been published. You will see from this that I could not have had the intention to deprive anyone of his just rights. [The rest of Professor ENCKE's letter is taken up with the development of one point of the method in a rigorous manner, to remove a doubt expressed by BOND in his paper.] . . .

Let us hope that the method will be often employed, so that gradually, step by step, we may come to understand our solar system. With the highest respect, Your obedient,

J. F. ENCKE,
Director of the Berlin Observatory.

TO HON. WILLIAM MITCHELL, NANTUCKET, FROM GEORGE BOND.

CAMBRIDGE, July 6, 1857.

Dear Sir:—

I have for some months past been engaged in preparing for publication my reports on the Coast Survey Chronometric Expeditions, and my anxiety to get finally through with them has prevented me from writing to you earlier with relation to the photographic experiments which have been made and are now in progress at our observatory. Your request to be kept informed in matters of this kind I will now comply with, in season, I hope, to be of service in posting up MARIA* on the subject before she leaves for Europe.

*** Miss MARIA MITCHELL, afterwards Professor in Vassar College.**

As far as I am informed, the attempt to photograph the fixed stars by their own light has been made nowhere else up to the present date. The rumor of a daguerreotype of a nebula, made in Italy some years since, was unfounded, and worth about as much as P. SEECHI's so-called lunar photograph of more recent date,—a photographic copy of an India ink drawing!

About seven years since (July 17, 1850,) Mr. WHIPPLE obtained daguerreotype impressions from the image of α *Lyræ* formed in the focus of the great equatorial, and subsequently from *Castor*, thus establishing a simple, but not uninteresting fact—the possibility of such an achievement. On these occasions a long exposure of one or two minutes was required before the plate was acted upon by the light, and in this interval the irregularities of the Munich clockwork were so large as to destroy the symmetry of the images, while the smaller stars of the second magnitude would not "take" at all.

For some years after Mr. WHIPPLE gave his attention to photographs of the moon and sun, and the stars were left to themselves. But improvements in the art progressed rapidly; the preparations were more sensitive, the artists had acquired more experience. At the same time the principle of the spring-governor had been thoroughly tested, and found to supply a great desideratum in imparting a sidereal motion to the telescope incomparably more uniform than that attained by the Munich mechanism. If you have been present (I have not) at some of the more recent meetings of the American Association, you may have heard this same spring-governor (the same which regulates our electric apparatus) condemned in public—thoroughly used up, you may say,—by men who consider themselves high authorities. After being annihilated by the demonstrations of Professors PEIRCE, BARTLETT, GOULD, etc., it is inexcusably perverse in continuing to do its work perfectly, obstinately defiant of theory and mathematics, but conforming exactly with good common sense.

On page 294 of the Coast Survey Report, 1855, Doctor GOULD, in his anxiety lest the observatory of Harvard should gain any credit for the invention of the governor, commits a ridiculous blunder in comparing it with the Munich clock, which it resembles as it would anything else that has wheels —a wheelbarrow or handcart, for instance. But neither talking nor writing nor false reasoning will change the nature of what is true, and so our clock keeps pace with the stars perfectly, and we exult in a final triumph. But to return, Messrs. WHIPPLE and BLACK recommenced their trials on other images (taken by the collodion process) in March of the present year, and they are still in progress. The expense of time, chemicals, etc., is far more considerable than one would have anticipated—each night, in fact, opens new vistas requiring exploration. The field for experiment is too vast to be at once occupied, even if we were provided with unlimited means. But the results already obtained in the disconnected attempts we have thus far been enabled to make, are of the highest interest, and suggest possibilities in the future which one can scarcely trust himself to speculate upon. Could another step in advance be taken equal to that gained since 1850, the consequences could not fail of being of incalculable importance in astronomy.

The same object, α *Lyræ*, which in 1850 required 100^s to impart its image to the plate, and even then imperfectly, is now photographed *instantaneously* with a symmetrical disc perfectly fit for exact micrometer measurement. We then were confined to a dozen or two of the brightest stars, whereas now we take all that are visible to the naked eye. Even from week to week we can distinguish decided progress.

Of the beauty and convenience of the process you will scarcely form a correct idea without witnessing for yourself, [which I hope you will be able to do before long.]

On a fine night the amount of work which can be accomplished, with an entire exemption from the trouble, vexation and fatigue which seldom fail to attend upon ordinary observations, is astonishing.

The plates once secured, can be laid by for future study by daylight and at leisure. The record is there, with no room for doubt or mistakes as to its fidelity. As yet, however, we obtain images only from stars to the sixth magnitude, inclusive. To be of essential service to astronomy, it is indispensable that great improvements be yet made, and these, I feel sure, will not be accomplished without a deal of experimenting. To do this properly we need for at least a year to come the services of the excellent artists who have hitherto literally given us their assistance, expensive materials and instruments. They should be liberally remunerated, and feel at liberty, when the prospect is good for a fair night, to give up their day's business and come to the work fresh and fit to spend the whole night at the telescope. As matters are at present, they come to the observatory thoroughly exhausted, for it generally happens that the best nights are preceded by their busiest days. They make no charge for their time, costly chemicals and instruments, and as they are volunteers, we have no claim on them, and cannot, in conscience, require more of men utterly exhausted than they have done. But could we but press this matter on, we should soon be able to say what we can and what we cannot accomplish in stellar photography—the latter limits we certainly have not reached as yet. At present the chief object of attention must be to improve the sensitiveness of the plates, to which I am assured by high authorities in chemistry there is scarcely any limit to be put in point of theory. Suppose we are able finally to obtain pictures of seventh magnitude stars. It is reasonable to suppose that on some lofty mountain and in a purer atmosphere we might, with the same telescope, include the eighth magnitude. To increase the size of the telescope threefold in aperture is a practicable thing if the money can be found. This would increase the brightness of the stellar images, say eightfold, and we should be able then to photograph all the stars to the tenth and eleventh magnitude, inclusive. There is nothing then so extravagant in predicting a future application of photography to stellar astronomy on a most magnificent scale. It is even at this moment simply a question of finding one or two hundred thousand dollars to make the telescope with and to keep up the experiments.

What more admirable method can be imagined for the study of the orbits of the fixed stars, and for resolving the problem of their annual parallax than this would be if we could obtain the impressions of the telescopic stars to the tenth magnitude! Consider, too, that groups of ten, or fifty even, if so many occur in the compass of the field, will be taken as quickly as one alone would be, perhaps in a few seconds only, and each mapped down with unimpeachable accuracy!

It would be useless for me to attempt to describe in a letter the processes and results in detail. Can you not come up and see for yourself? I am going away about the 15th of August, and till then we shall be at work on the subject. Please say to

MARIA that two or three copies of our volume on *Saturn* will be sent to her as soon as they come from the binders (they were promised to-day), and some veritable star photographs. Please give my regards to her and to other members of your family, and believe me, Very truly yours,

G. P. BOND.

HON. WM. MITCHELL.

P. S.—I find I have forgotten to allude to two important features in stellar photography—one is that the intensity and size of the images taken in connection with the length of time during which the plate has been exposed measures the relative magnitudes of the stars. The other point is, that the measurements of distances and angles of position of the double stars from the plates, we have ascertained by many trials on our earliest impressions, to be as exact as the best micrometric work. Our subsequent pictures are much more perfect, and should do better still. G. P. B.

SIR G. B. AIRY TO GEORGE BOND.

ROYAL OBSERVATORY, GREENWICH,
LONDON, S. E., 1857, November 6.

Dear Sir:—

I have to thank you very heartily for a paper on the use of equivalent numbers in the application of the method of least squares. I had used the method myself on some few occasions, but I never published anything on it, and so far as I know no other person has published it.

Your paper, however, has delighted me; it is the most reasonable treatise on the subject that I have seen for a long time.

The example of the substitution of round numbers for exact numbers is very clear. The exhibition of the great alteration produced in the resulting roots of the equations, which, as you demonstrate, are *very* nearly as trustworthy as those given by the exact theoretical numbers, will at first shock many readers, but cannot fail ultimately to interest them. I am, dear sir,

Your faithful servant,
(Signed) G. B. AIRY.

FROM J. HOMER LANE TO GEORGE BOND.

WASHINGTON, D. C., October 23, 1858.

Dear Sir:—

It has occurred to me, and independently to Mr. TAYLOR, a chief examiner in the Patent Office, that a stereoscopic image of the moon might be obtained by taking advantage of her librations.* Not only might we expect the stereoscopic effect of solidity, but Mr. TAYLOR well suggests that a stereoscopic view, by the increased power of interpretation it will afford, may even lead to new discoveries respecting the physical peculiarities of the moon's surface. We think, therefore, the attempt to multiply stereoscopic views will be well worth considerable pains, and understanding that you have been paying attention to the production of photographs of the moon, we thought proper to address you, with the hope that you may have it in your way to make experiments on the subject. It is a matter which will, of course, require considerable time, it being necessary to select two corresponding epochs, which, while they present to the observer in sufficient degree the effect of libration, also have the sun at the zenith of the same point of the moon's surface, that the illumination may be identical. I

[*See a paper by Doctor DE LA RUE, in *Report* B. A. A. S., 1859, p. 148.]

need not enlarge on details, the mere suggestion being sufficient. It may not be impossible to make a selection from pictures already obtained, if they are sufficiently numerous.

Respectfully yours,
J. HOMER LANE.

TO DR. J. H. ARMSBY, TRUSTEE OF THE DUDLEY OBSERVATORY, ALBANY, FROM GEORGE BOND.

CAMBRIDGE, January 20, 1859.

Dear Sir:—

I am fully sensible of the honor conferred by the invitation of the trustees of the Dudley Observatory, communicated in your letter of the 17th inst., but must decline accepting it.*

The present board of trustees I consider perfectly competent to manage all the affairs of the institution properly coming under their charge. An astronomer fit to direct its scientific operations can stand in no need of a special "scientific council," and may find their control offensive or otherwise troublesome.

You will readily infer from the expression of these views the reasons of my unwillingness to act as a member of the proposed conference. But that there may be no room for misapprehension, allow me to add that I have full confidence in the judgment of the gentlemen named in your letter, and believe that Professor MITCHEL, who is understood to be the choice of the trustees as director, will fill

* Doctor ARMSBY's letter is not to be found among BOND's papers. It was an invitation to be present at Albany at a conference between the trustees of the Dudley Observatory and invited astronomers, to consult upon the future policy of the Dudley Observatory, from the directorship of which Doctor GOULD had lately been removed.

that office with honor to the observatory and to the satisfaction of those who have contributed to its establishment. Very truly,

G. P. BOND.

FROM PRESIDENT WALKER, OF HARVARD COLLEGE, TO GEORGE BOND.

QUINCY STREET, February 28, 1859.

Dear Sir:—

I have the pleasure to inform you that the corporation on Saturday elected you to succeed your father as Director of the Observatory and Phillips Professor of Astronomy. The election must be confirmed by the Overseers, who will meet on Wednesday, March 9th.*

Faithfully yours,

JAMES WALKER.

GEORGE BOND TO HON. WM. MITCHELL.

March 10, 1859.

. . . .

Now that my position at the observatory is settled, I wish to compose matters, if possible, with Professors PEIRCE and BACHE, so far as can be without a compromise of independence. This contention and perpetual hostility of interests is a miserable occupation for men who are capable of better things. G. P. BOND.

* The election was duly confirmed; BOND was, at this time, thirty-four years of age.

To Professor Benjamin Peirce from George Bond.

Observatory of Harvard College,
March 12, 1859.

Dear Sir:—

You do not need to be reminded that the mutual confidence and friendship which once subsisted between us has for some time past been disturbed, if not wholly interrupted.

There can be little use in dwelling upon the circumstances which have occasioned this unfortunate difference; perhaps it would be better if they were forgotten.

My object now is to propose and to open the way for a return to a better state of feeling.

No one can appreciate more highly than I do the advantages which would accrue to the observatory from your coöperation with it. Both your position and attainments are such as to enable you to render most valuable aid, and they must always ensure a respectful consideration of your opinion and advice.

Any assistance which I can give in aid of your scientific investigations, by furnishing the results of observations, or in other ways, shall be freely extended, but a cordial and earnest coöperation can be brought about only by a mutual consent to give up past differences. On my part nothing shall be wanting to accomplish that end.

Respectfully yours,
G. P. Bond.

[No reply to this letter is to be found in Professor Bond's papers, and it is believed that none was received].

Among many letters of congratulation the following, from the Hon. Edward Everett, formerly

President of Harvard College, is printed as representative:—

FROM HON. EDWARD EVERETT TO GEORGE BOND.

BOSTON, 14th of March, 1859.

My Dear Sir:—

Your favor of the 8th came duly to hand while I was in New York. I am greatly indebted to you for it, and will endeavor to make use of the materials so kindly furnished by you in such a way as not to discredit your father's memory.

I hope it is not too late for me to congratulate you on being chosen in his place. I took a great interest in the election, which at one time seemed to be threatened with opposition, and in common with other friends exercised what influence I possessed to procure a right result, which, however, I must say, was brought about by the merit and undoubted qualification of the successful candidate.

Wishing you much happiness and continued success in your labors, I remain, dear sir,

Sincerely your friend,

EDWARD EVERETT.

FROM PROF. C. C. FELTON TO GEORGE BOND.

CAMBRIDGE, March 23, 1859.

My Dear Sir:—

Mr. BUTLER* stated in his speech in opposition to the grant for the museum, that "the people of the State had built the observatory; but that though it was built with their own money, it had been shut in their faces, and that a letter had been written by a member of the legislature, asking permission

* BENJAMIN F. BUTLER?

to visit it, but the letter had never even been answered."

As all the first part of the statement I know to be a lie, I infer that the story about the letter is also. But I do not venture to contradict it without authority. Will you have the kindness to inform me whether any such letter has ever been received? I have some thought of answering his slanders generally.

Yours very truly,
C. C. FELTON.

TO PROF. C. C. FELTON FROM GEORGE BOND.

OBSERVATORY OF HARVARD COLLEGE,
March 24, 1859.

Dear Sir:—

In reply to your note of yesterday, allow me to say that no letter, to my knowledge, has ever been received by my father or by the authorities of the college, certainly none by myself, from any member of the present legislature, asking permission to visit the observatory. A communication or an intimation of the kind addressed to either of us would assuredly have met with proper respect.

It is possible that among the numbers whose curiosity to see the famous comet of October last, through the great telescope, was unavoidably disappointed, there may have been members of the legislature, though I can recall no such instance; but they have no reason to complain of their exclusion on an occasion when both our college officers and our contributors, who, and not "the people of the State," have built the observatory, uniformly manifested their consideration and good sense by relinquishing greatly superior claims.

In both the particulars mentioned in your note, Mr. BUTLER exposes himself to a rebuke, which I hope you will not omit the opportunity of administering.

Yours truly,
G. P. Bond.

To Prof. W. A. Norton from George Bond.

May 5, 1859.

Dear Sir:—

Your letter of the 12th ult. came duly to hand. I agree with you, that the axis *stripe* of Donati does not accord with the idea of a tail formed like a cone with a large hollow within. It is more probable that it is much flattened and was presented broadways to us. This, at all events, would best account for the sharp outlines of the stripe.

If we consider how small the nucleus is, and the amount of light reflected from the tail, which is constantly being replenished from the nucleus, it cannot be doubted that the particles of the tail must be exceedingly minute in mass, but quite considerable in the aggregate of their surfaces. It is equally evident that they are under the influence of some other force than gravitation.

We have, then, in the tails of comets an excessively minute subdivision of matter accompanied by a deviation in the motion of the constituent particles from the gravitation path. As the only necessary consequence of subdivision is increased surface, the simplest inference from the facts seems to be that surface as well as mass is concerned in the mutual action of the heavenly bodies. The surface force may be universal, but still, in most cases, insensible to our means of observation.

There are difficulties in the way of determining the mass of the nucleus as you propose. The force may not be common to all the particles of the sun and nucleus. And, moreover, the nucleus is not in the focus of a parabola, having its vertex at the vertex of one of the well-defined envelopes (though I think it approaches this condition for the outer nebulosity), and representing as nearly as possible the outline of the envelope. The outline of the latter, in its most definite stage, approaches to a semicircle on the side next the sun, with the nucleus in the center, rather than to a parabola. This fact, taken in connection with the uniform rise of the envelope towards the sun, looks more like a rise of strata of mist in an atmosphere than anything else, and does not accord with the theory on which you have computed the mass, if I rightly apprehend it.

I should add that the idea of a surface force was first suggested to me by a paragraph in your article, p. 102. Very truly yours,

G. P. Bond.

From George Bond to R. C. Carrington.

Observatory of Harvard College,
June 11, 1859.

Dear Sir:—

I have just received yours of the 26th ult., and am so much interested in the photograph which you had the kindness to send that I cannot help sitting down at once to thank you for the favor. Here is a very singular fact. The camera lens, with its short focus, affords a strong image of the *nebulosity* of the tail [of Donati's Comet] at a point where the *intensity* of the light was probably a thousand times less than that of the nucleus. And this, too, in seven seconds; whereas, with our object glass of probably eight or ten times the area, we barely obtained an impression of the *nucleus* itself in 360 seconds on the following day, September 28th. Assuming the camera to have 12-inch focus and five-inch aperture, the *intensity* of its image of an object would not be so much as sixty times greater than the intensity of an image of the same object in the focus of our object glass. Now, in the case of star images, I have found that the time of exposure is nearly in the inverse ratio of the intensity of the light—that is, by increasing the area of the object glass twice, the image forms in half the time, so that we should conclude that in the *camera* the nucleus would form an image, if of a sensible area, in $\frac{1}{60}$ of the time required in the large telescope, or in $\frac{360}{60} = 6^s$; but that the incomparably fainter nebulosity of the remote parts of the tail should give so distinct an image in only seven seconds is a very interesting fact.*

. . . .

I have long thought that there was a kind of sympathy in the photographic action, light acting at one point rendering neighboring points more sensitive, and that possibly a star image slightly out of focus might "take" more quickly than when reduced to a minimum area. If this thing be true, then a poor telescope would be better than a good one, which would be introducing a new principle in practical economy! †

. . . .

Yours very truly,
G. P. Bond.

---

* Until this letter became known, the historians of astronomical photography supposed the first comet photographs to be those of 1881.

† Bond's "principle" can be illustrated by photographing the Milky Way with objectives which are well and poorly corrected, respectively; or by using a good lens in focus and then slightly out of focus. The amount of false nebulosity increases with the badness of the lens employed or with the inaccuracy of its focusing.

To William Lassell from George Bond.

Observatory of Harvard College,
Cambridge, U. S., June 16, 1859.

Dear Sir:—

. . . .

It is gratifying to hear that your giant telescope is so far advanced. Can you not make some provision, while it is being completed, for applying it to celestial photography? . . .

With my best remembrances to the members of your family,

Very truly yours,
G. P. Bond.

From Dr. F. Bruennow to George Bond.

Ann Arbor, June 23, 1859.

Dear Sir:—

Doctor Peters writes to me that you complained to him of the scarcity of good standard stars for your zones.

It would give me great pleasure if my observations can be of any service to you, and if you send me a list of such stars as you want to Albany, I will determine their positions carefully, as soon as they can be observed in the meridian. With greatest regard,

Yours ever truly,
Bruennow.

To General Edward Sabine from George Bond.

Observatory of Harvard College,
Cambridge, U. S., June 24, 1859.

Dear Sir:—

. . . .

In a small book "On the Calculation of Solar Eclipses," by Whiston, London, 1724, there is an account of some determinations of dip and variation made at Boston, N. E., in 1722. WHISTON promised the publication of the details, and says that the original journals are in the hands of "SAMUEL MOLYNEUX, Esq., Secretary to the Prince of Wales, and Fellow of the Royal Society."

Do you know whether these or any other as early determinations of the dip in America are accessible, whether in print or in manuscript?

WHISTON gives 74° 45′ as the dip at London, and 68° 22′ at Boston; about 6° less than our present value. His accounts, however, are obscure as to the degree of reliance which can be placed on the results. Respectfully and truly yours,

G. P. BOND.

GENERAL SABINE TO GEORGE BOND.

LONDON, July 18, 1859.

My Dear Sir:—

. . . I will not fail to make the inquiry you suggest about WHISTON'S original journals for dip and variation at Boston in 1722. His (WHISTON'S) dip in London, in 1720, agreed remarkably well with GRAHAM'S determination in 1724, which was 74° 42′. I remain faithfully yours,

EDWARD SABINE.

TO DR. C. H. F. PETERS FROM GEORGE BOND.

OBSERVATORY OF HARVARD COLLEGE,
CAMBRIDGE, July 12, 1859.

Dear Sir:—

. . . .

The comet photograph is a good illustration of the importance of experimenting further in this field. With our large telescope we could only procure an image of the nucleus in 6^m, while this artist, with a small camera, got a picture of the vastly fainter portions of the tail in but a few seconds.

The arrangement by which both MITCHEL and BRUENNOW remain directors, each of two observatories, seems an odd one. . . .

Very truly yours,
G. P. BOND.

FROM PROF. W. A. NORTON TO GEORGE BOND.

NEW HAVEN, July 15, 1859.

Dear Sir:—

I learn that Professor PEIRCE, at a recent meeting of the Boston Academy, communicated a determination of the density of the nucleus of DONATI'S Comet, and criticised my calculations or theoretical views. Will you do me the favor to inform me of the substance of his objections? I do not see how he can have made such a calculation without making use of my theory of repulsion exerted by the nucleus. I take it he does not claim to be the author of this theory. I understand that he makes the density of the nucleus of DONATI'S Comet 9, instead of from 5 to 6, as I do. . . .

Very truly yours,
W. A. NORTON.

FROM REV. W. R. DAWES TO GEORGE BOND.

HOPEFIELD LODGE,
HADDENHAM THAME, 28th July, 1859.

My Dear Sir:—

As our friend, Mr. ALVAN CLARK has, to our great regret, left us, and sails from Liverpool on

Saturday, the 30th, I am requesting him to take charge of the accompanying rough copies of my sketches of DONATI's Comet.

. . . .

I am greatly obliged to you for the copy of your "Account of DONATI's Comet." I have perused it with intense interest. The illustrations are also excellent.

I feel much gratified that you consider my observations of the comet useful. . . . I am greatly pleased with the new equatorial which Mr. CLARK has brought me and erected in my observatory. The ingenious application of your esteemed father's "spring-governor" seems to produce an equability of movement in the driving clock much superior to anything which I had previously seen.

Believe me to remain, my dear sir,

Yours very truly,

W. R. DAWES.

FROM PROFESSOR JOSEPH HENRY TO GEORGE BOND.

CAMBRIDGE, August 26, 1859.

My Dear Professor:—

. . . .

If you find you cannot procure the means of preparing and publishing your paper on the comet, write to me on the subject, and I will make an arrangement for the expenses of the work.

It would give me much pleasure to see you or any of your family at Washington.

With kind regards to your mother, brother and sister, I remain, Very truly your friend,

JOSEPH HENRY.

TO DR. C. H. F. PETERS FROM GEORGE BOND.

OBSERVATORY OF HARVARD COLLEGE,
CAMBRIDGE, Mass., September 27, 1859.

Dear Sir:—

. . . .

I have just written to GILLISS on the subject of the new parallax expedition*—that I have no confidence that more will come of it than a satisfactory confirmation of ENCKE's value. If the twenty-eight direct comparisons with northern micrometer observations (cited in the *Report Astr. Ex.*, p. lxx) are deserving of no confidence (see the remark at foot of p. lxx), then ten times as many, equally bad, would not contribute any important accession to our present knowledge of this element.

Very truly yours,
G. P. BOND.

TO AUGUST SONNTAG† FROM GEORGE BOND.

OBSERVATORY OF H. C., January 9, 1860.

Dear Sir:—

Enclosed you will find observations of *Mnemosyne*, with which your ephemeris agrees as well, probably, as you could hope.

. . . .

If it were not too much like a waste of time, it might be well to overhaul the report on the Chili Expedition.

The assertion that its failure is due to the want of northern coöperation has not the slightest foundation. The thirty results, on p. lxx, must be reckoned as fair specimens of the work which an unlimited

* Proposed at this time, but not carried out.

† Assistant Astronomer at the Dudley Observatory; Astronomer to Doctor KANE's Arctic Expedition, 1853-55.

coöperation could accomplish. Now it is a fair statement to say that if thirty observations of average goodness have "such a large mean error and striking nonaccordance as to render any reliance upon them impossible" (*Rep.*, p. lxx), then an unlimited number will not furnish a result appreciably better than the first thirty.

Look at the diameters of the wires of the Santiago equatorial by daylight and at night. The diameters of *Mars* and *Venus!* The rejection of the Athens observation, because Doctor GOULD has applied the parallax with a wrong sign, and the wholesale rejection of northern and southern observations together, is the adopted result.

But I have no room for more.

Truly yours,
(Signed) G. P. BOND.

For your own edification, just assume the principles for the assignment of weights given in *Rep. Chili Exped.*, p. ccliii — which seems reasonable,— and try how much the weight of the final result could have been increased by an infinite number of northern observations.

Is it not as follows?

Northern Obs.	239.	Southern	564	Act. Wt.	591
" "	∞	"	564	"	598
" "	∞	"	∞	"	600

TO DOCTOR F. BRUENNOW FROM GEORGE BOND.

OBSERVATORY OF HARVARD COLLEGE,
February 21, 1860.

Dear Sir:—

. . . .

The results of our photographic experiments conduct to a very interesting suggestion — viz. that *Jupiter* gives strong indications of analogy to the sun in physical constitution of atmosphere, and is, *chemically* speaking, self-luminous, or approaches that condition so remarkably that this inference is the most probable one that can be drawn.

The comparison of the photographic peculiarities of *Jupiter*, sun and moon led me to inquire into the analogies existing between the belts and the spots of *Jupiter*, and the distribution of the solar spots in zones or belts. Both bodies have faculæ. The resemblances in point of distribution and proper motion are very striking. The idea then occurred, that if chemical action were going on at the surface of *Jupiter*, there would be a reflex action between it and the sun; the sympathy we recognize so universally in other connections. I found that the maxima and minima of the solar spots occur at *about* the perihelia and aphelia of *Jupiter* as far back as 1826, but beyond this there is no satisfactory coincidence with WOLF'S period. In conversation with Mr. SAFFORD, I had planned an investigation of the representation of the solar spot curves by periodic terms of sine and cos, of true anomalies of all planets, the coëfficients to vary as $\frac{1}{r^2}$. To my surprise, last night I found that WOLF had, from altogether different considerations, suspected some such relations.

Optically *Jupiter* has certainly a higher intrinsic brilliancy than it should have. I estimate it at $\frac{1}{2.5} = \frac{2}{5}$ of that of the moon. Next full moon I will test this by a better method. I wish you would make some estimates.

There is another very singular matter bearing on this question. For ten or twelve years past my father and myself have studied the phenomena of the transits of *Jupiter's* satellites. We have seen them projected on the disc as *black* as the shadows. It is impossible to admit the explanation that this results from dark spots on the satellites, for reasons which I cannot stop to explain, but which will readily occur to you when you study, and above all *see*, the phenomenon. The first satellite I have never seen black, but only dusky or dark; when on a dark belt, it disappears. All the satellites enter and pass off bright, even when fairly entered on the disc, showing the great contrast between the central and marginal parts of the disc in point of brilliancy. The eye alone is quite unequal to recognizing the full strength of the contrast.

There is one objection to the hypothesis of *Jupiter's* self-luminosity, besides the intensity of the shadows of the satellites, and that is their complete disappearance in eclipse, though they should be illuminated by the native light of *Jupiter* on the side opposite the sun. However, I find the amount of this illumination is small, although the area of *Jupiter's* surface presented to the first satellite is 1400 times that of our full moon, yet the total quantity of light reflected will amount at best to only $\frac{1}{37}$ of that which the satellite gets from the sun. However the question of optical or visible self-luminosity may turn out the chemical side of the question is still stronger.

I mean to try whether the aurora, which is the earth's native light, has not a similar property, viz. more of chemical than of luminous energy, as we should anticipate from analogy, if *Jupiter's* light be auroral.

I do not advance any theory on the subject, but the facts are curious, and should direct attention to a comparison of the aspects and the two bodies of *Jupiter* and sun. Yours very truly,

G. P. Bond.

Compare Vaughan's and Thompson's theories, in which all bodies of great mass and sufficient density should be suns. It was this that first led me to try our late photographic experiments.

TO DR. F. BRUENNOW FROM GEORGE BOND.

March 20, 1860.

Dear Sir:—

I quite agree with you in withholding entire confidence in the new planet discovery . . . without more evidence.*

. . . .

What a discouraging search Mr. TUTTLE has had for comets for eighteen months past. He has been constantly at work during the whole time. . . .

GEORGE BOND.

FROM E. C. HERRICK TO GEORGE BOND.

YALE COLLEGE, NEW HAVEN,
March 26, 1860.

Dear Sir:—

I am very desirous to hear if you have seen the planet *Vulcan* this afternoon. From 4^h 55^m onward to sunset clouds wholly intercepted our view. From 3 P. M. to 4^h 55^m we had several fair views of the sun, amid the gathering clouds, but among the many spots there was no apparent planet. I hope you had a better sky.

For nearly a month past four or five of us have been watching the sun in the hope of detecting the transit of LESCARBAULT's inter-mercurial planet. With the uncertainty as to the exactness of the period of 19.7 days, I thought it best to observe during March and the early part of April. But since receiving the data which WOLF has brought to light there appears to be strong ground for supposing the period to be 19.27 days. This gives a *possible* transit March 7th, and a *probable* one to-day

* LESCARBAULT'S *Vulcan.*

at 5:30 P. M. of our time. The former day was clouded here wholly, but an observation may have been secured in Europe. The transit of to-day was, of course, invisible there.

. . . .

On the 8th inst. I commenced a letter to you relative to the subject of watching for this planet, asking more particularly that if you intended searching therefor you would look especially between 9 A. M. and 3 P. M., when our telescope is unfortunately unable to command an altitude sufficient to reach the sun, but hesitating to trouble you in the business, I threw the sheet aside. I now regret that I did not last week call your attention to the probable transit of to-day, lest the matter should have escaped your notice.

Yours truly,

EDWARD C. HERRICK.

P. S.—In our ignorance of the ellipticity of *Vulcan* there must be some uncertainty as to the day of the March transit, and I propose that we continue the watch for a week longer.

The following extracts from a long report exhibit the condition and requirements of the Harvard College Observatory at the beginning of GEORGE BOND's activity as director. The notes in [ ] show the final dispositions made up to 1897.

TO J. INGERSOLL BOWDITCH FROM GEORGE BOND.

OBSERVATORY OF HARVARD COLLEGE,
CAMBRIDGE, March 31, 1860.

Dear Sir:—

To place the observatory in a position of complete efficiency, we need, as you well know, a considerable addition to our funds, larger, perhaps, than we can expect to obtain at present.

The most urgent necessity relates to the subject of printing. For the last twelve or thirteen years the great refractor has been in constant service, and has furnished an extensive collection of observations in those departments of astronomy which afford the most appropriate field for the employment of an instrument of its class. The material thus accumulated acquires peculiar interest and importance from the superior size and excellent qualities of the telescope. There are, besides, contributions from the less noted instruments—the transit, small equatorial, etc., which should be included in the plan of publication.

I will mention specifically the following as subjects for future volumes. Those marked with an asterisk are in a condition of forwardness, as respects arrangement and discussion of the original notes, which would admit of their being put immediately, or with little delay, into the hands of the printer or engraver. Others will require more labor in their preparation, though the observations themselves are complete, with the exception of the third series of zones, now in progress.

SUBJECTS FOR PUBLICATION.

* Zone Catalogue of 5000 Stars, between Dec. $+0° \, 20'$ and $+0° \, 40'$. [*Annals* H. C. O., Vol. II, part 2 (1867).]

Zone Catalogue of 5000 Stars between Dec. $+0° \, 40'$ and $+1° \, 00'$ (observations now in progress). [*Annals* H. C. O., Vol. VI (1872).]

* Meridian Transits, Moon Culminations, and Standard Catalogue. [*Annals* H. C. O., Vol. IV, parts 1 (1863) and 2 (1878).]

* Observations on the Solar Spots. (These consist principally of drawings, to the number of two

hundred and fifty, executed in 1847–48–49 by the late director.) [*Annals* H. C. O., Vol. VII (1871).]

Observations on Comets, Asteroids, Double Stars, Nebulæ, and Miscellaneous Observations.

*Observations and Drawings of the Great Nebulæ of *Orion* and *Andromeda*, and of the Cluster in *Hercules*. [*Annals* H. C. O., Vol. V (1867).]

Observations on the Physical Aspect of *Venus*, *Mars* and *Jupiter*, and on the Satellites of *Jupiter*. The drawings of *Jupiter* will number nearly 150, showing details of the structure of the belts during the past twelve years.

Determination of the Solar Parallax from Observations of *Venus* and *Mars;* Satellites of *Neptune* and *Saturn;* Specimens of Astronomical Photography, Stellar Photography, etc.; Meteorological and Magnetic Observations.

Eclipses and Occultations, including an account of the total eclipse of the Sun, observed at Lilla Edet, Sweden.

Description † and Drawings of the Great Comet of DONATI, with illustrations from other bodies of this class. [Printed in Vol. III of the *Annals* H. C. O. (1862).]

The foregoing, with other materials not mentioned, would fill eight or ten quarto volumes of 300 or 400 pages.

Provision for meeting current expenses, such as fuel, lights, stationery, repairs, care of grounds and buildings, etc.

We cannot now afford fuel to heat our library in the winter, and I often put on my great-coat,

† During the administration of Professor WINLOCK an examination of all the records of the observatory was made, and with the publication of Vol. VIII of the *Annals*, in 1876, it was considered that all the available work of the observatory before 1866 had been printed. See the *Annals*, Vol. VIII, page 47.

and sit in an out-of-doors' temperature, when I want to consult the books. The expense of keeping the grounds in neat condition is borne by the director.

In the event of the erection of any new building or instrument, as for instance, of a telescope and building expressly designed for experiments in photographing the heavenly bodies, or of a small magnetic observatory, etc., there would be occasion for additional outlays.

Without provision for the above, the observatory cannot be considered as being in a position of full activity, and is not on an equal footing with similar institutions in Europe, which are mostly under government patronage.

. . . .

I shall now mention several particulars, in respect of which the operations of the observatory might be extended to great advantage, if the means were at command.

First—A new meridian instrument of the best construction—estimate, $6000.

Second—A small magnetic observatory, $1000.

Third—It would be a great attraction to the students of the University, and calculated to exercise a most beneficial influence upon their education, if they could have free access to such facilities for the exploration and study of celestial phenomena as an observatory affords. By an arrangement which has been in force for twelve years past, the Senior Class of Undergraduates have made an annual visitation to the observatory, in five or six divisions.

This provision is a very inadequate one to meet the wants of the students; . . . but ineffectual as it is to satisfy their reasonable desires, it is at the same time impossible to give them larger privileges without encroaching most seriously upon the regular work of the observatory.

. . . I propose for a remedy that a telescope of the finest quality and largest dimensions—superior, if possible, to the great refractor now the chief ornament of the observatory—should be mounted on the grounds and devoted to the instruction of the students, primarily in the various objects of planetary and stellar Astronomy.

. . . .

If the students and other visitors were admitted only at certain days and hours, much valuable use might be made of the telescope by having reference in its construction and mounting to photographic purposes, for which it could be employed, perhaps, on certain days, or after midnight or some other late hour.

Fourth—The application of photography to astronomical purposes stands greatly in need of further development. It has already afforded most valuable results, and there is every reason to suppose that it will one day become a great power in the investigation of the most interesting problems of astronomy. The experiments must be made on a liberal scale, and the whole time of an artist should be devoted to the subject.

. . . .

It would be certain to repay the outlay if an astronomer of experience, furnished with a good telescope and photographic apparatus, should visit different parts of the world (high table-lands and mountains), and experiment on the advantages of a pure and tranquil atmosphere. It is understood that photography succeeds better in California than here, and better here than in Europe. Now a few essays in California, at an expense of a few thousand dollars, might prove of inestimable advantage to the science. The Russian Government has just appropriated nearly $40,000 for an expedition to Persia, to try the effect of a pure atmosphere on the visibility of celestial objects, but they make no mention of the most important means of extending our researches, namely, photography. Why should we always have to wait for the example of the governments of Europe in encouragement of scientific enterprises? If our observatory had possessed the means, we should have sent off an expedition of this kind years ago; it was actually proposed, but, of course, nothing could be accomplished without money. We might now, with equal means, get our expedition to the interior of California, secure the best of the results and get back before the Russians have started.

The last matter which I will dwell upon relates to further provision for the support of the observers attached to the institution.

. . . .

There are three, and at times, four assistants; their annual compensation averages less than $500 a year. I am ashamed to have them remain for such a miserable pittance, but do not know how to keep up the work expected from the observatory without them. If this is to be extended, and additional instruments brought into use, there must be provision made for the proper support of the astronomers.

The observatory can now depend, for its ordinary expenses of all description, the salaries of five observers, the purchase of books and instruments, fuel, lights, stationery, repairs, etc., upon an annual income of $5200, which is about half as much as is needed.

. . . .

Thanking you most cordially for the interest you have always manifested in the welfare of the observatory, I remain yours, very truly,

G. P. Bond.

RECAPITULATION.

ITEMS MOST NEEDED TO SECURE PRESENT EFFICIENCY OF OBSERVATORY.

1st—Printing and engraving of arrears of observations in past years, ...	$20,000
2d—For annual publication of *Annals*, fund giving income of $2,500, ...	50,000
3d—Fund for contingent expenses, income $500 a year,	10,000
	$80,000

ITEMS DESIRABLE FOR INCREASING ITS OPERATIONS.

1st—A new meridian instrument of the best construction, estimate, ...	$ 6,000
2d—A small magnetic observatory, ...	1,000
3d—Cost of a telescope and tube of the largest size and best quality, ...	12,000
Mounting, clockwork, micrometer, etc., ...	5,000
Building, pier, observing chair, etc., ...	5,000
Annual compensation of assistant, $500 per annum. Fund, ...	10,000
4th—Photographic experiments for five years, $1,000 per year, ...	5,000
Compensation of artist for same period, at $1,000 a year, ...	5,000
5th—To increase present income by $5,000. Capital, . .	100,000
Total, ...	$229,000

To G. B. Airy from George Bond.

September 28, 1857.

. . . My father requests your acceptance of the enclosed plate exhibiting a line photographed by α *Lyræ*, as it passed the field of the equatorial by its diurnal motion, the telescope remaining fixed. You will perceive that your idea for the self-registration of transits and zenith distances can be perfectly realized for this particular star.

You will notice considerable fluctuations in the intensity and regularity of the line, to be ascribed partly to changes of refraction, partly perhaps to the fact that contiguous portions of the plate are not equally sensitive to the action of light; but it is possible that the phenomenon of *twinkling* may have some part in determining the character of the line traced by the star.

It seems now highly probable that, by taking advantage of the aids which the art of photography has placed within our reach, a grand impulse might be given to astronomy. By the employment of electro-magnetism the astronomer has been relieved from dependence upon the sense of hearing and the mental faculties which used to be called into exercise in estimating small fractions of time. Photography may be enlisted in aid of, or even as a substitute, for the eye.

It is to be supposed that these first attempts are susceptible of great improvement. We can certainly have larger telescopes. I believe that a liberal expenditure of money will solve all the mechanical difficulties in the way of constructing a telescope of four times the aperture of ours. We should then photograph *instantaneously* stars $\frac{1}{16}$ as bright as *α Lyræ* and, as I estimate, in less than one minute, stars of the 8th or 9th magnitude, without reckoning upon any improvement in the preparation and treatment of the plates. Certainly we have a right to expect much from chemistry in furtherance of the same end.

We have also yet to try the effect of using the telescope in a purer atmosphere; on an elevated mountain, for instance. . . .

To R. C. Carrington from George Bond.

February 29, 1860.

. . . I noticed the unexpectedly rapid action of *Jupiter* in photography on the first occasion that it was taken, nine or ten years ago. On March 22, 1851, I find the following memorandum referring to the time of exposure of the plates. It is noted that it was "about as long as the moon required, or not much longer." We have since confirmed this on several occasions, and DE LA RUE comes to much the same conclusion. Within a month or two past, I have engaged Mr. WHIPPLE to photograph the sun, moon and *Jupiter*. The central regions of sun and *Jupiter* have decidedly the most intensity, whereas the margins of the moon act first. A photograph of a cannon ball painted white was more uniform in action than either of the three above-named objects, but it most resembled the moon. ARAGO mentions a similar result for flat discs. *Mars* is brightest on the limbs, and *Venus* also; but the latter, not being seen in opposition, does not furnish as safe a comparison as *Mars* or the moon. Thus far, then, we find that *Jupiter* and the sun rank together in the *distribution* of light on their surfaces.

Sunlight at *Jupiter* is $\frac{1}{27}$ of the intensity that it is at the earth or the moon, and we should expect, with a similar reflecting surface, that *Jupiter* would show only about $\frac{1}{27}$ of the chemical intensity of the moon, whereas its central parts almost equal the average surface of full moon. (The intensities are in the ratio of 4 to 5, about.) So we must suppose *Jupiter* to be either *chemically* self-luminous, or else that it has a very peculiar property of light, reflection affording $\frac{4}{5} \times 27 = 22$ times actinic intensity of moonlight. This may be possible; its surface presented to us is gaseous, the moon is solid.

The spots, or faculæ, on *Jupiter* remind us of the sun, and it is curious to notice how the discordances in the sun's rotation from proper motion of spots is paralleled in the case of *Jupiter's* rotation, which fluctuates between $9^h\ 50^m$ and $9^h\ 56^m$, a very decided inequality. Then again, the question of the recurrence of spots in the same region is not established, though suspected with both bodies.

The arrangement of sun spots in zones and their prevalence in low latitudes, accords with *Jupiter's* belt action.

I have seen *Jupiter's* bright regions mottled like the sun, and compared the two to each other, years ago. *Jupiter's* spots are sometimes not dusky simply, but *black*, at any rate so-called, though we must not lay too much stress on an expression adopted perhaps hastily. The transit of *Jupiter's* satellites as black spots I have often witnessed. These always enter and pass off *bright*, as seen projected on the margin of the disc. The old explanation, which attributed the appearance to dark spots on the satellites, will not meet the facts, by any means. The first satellite is not black, or even always dark, excepting on a bright zone.

The grand objection to the idea of *Jupiter's* self-luminosity is the darkness of the shadows cast by the satellites, but it is a question how much defect of light in an object projected on a bright background will cause it to look black, especially when it subtends only a very small angle.

Perhaps if the side of *Jupiter* turned away from the sun be bright, we ought to see the satellites in eclipse. Here is another difficulty. Then again, SEIDEL makes *Jupiter*, *Venus* and *Mars* have equal "albedo," which militates against the idea that *Jupiter* is any more self-luminous than *Venus*, for instance. I think there must be an error in the *Monthly Notices* of the Astronomical Society for January, 1860, page 102, as regards the albedo of *Saturn* and *Mars*. The latter is fainter than *Saturn*, allowing for distance from sun and areas of illuminated surface.

I have been looking into the relation between the period of sun spots and the position of the principal planets as, in case of extensive chemical action going on in their atmospheres a reflex action on the sun, a sympathy between them, might be anticipated. At first all was promising up to 1826, as *Jupiter's* perihelia and aphelia answer tolerably to maxima and minima of spots, but previous to 1826 there is too much divergence. . . .

At all events, *Jupiter* should be studied henceforth with special reference to the relation between the physical constitution of its atmosphere and that of the sun. . . .

To Professor Alexis Caswell from George Bond.

Observatory of Harvard College,
May 14, 1860.

Dear Sir:—

The report of the early success of photographing stars at Rome may be true, but I know of two instances of supposed photographs of celestial objects taken at the Collegio Romano proving to be photographic copies *from drawings.* One of these is alluded to by Mr. Hunt, in his *Researches on Light,* as an instance of the direct impressions of the light of the nebula of *Orion.* The other is a copy from an india-ink drawing of the lunar crater *Coperniсus.* If the Roman astronomers procured veritable photographs of any of the celestial bodies before the experiments of Mr. Whipple at Cambridge, the accounts have no doubt been published, but I have never seen or heard of them, excepting as above stated.* Very truly yours,

G. P. Bond.

*** No photographs were made at Rome before those made by Bond and Whipple.**

To Mr. Black from George Bond.

Observatory of Harvard College,
Cambridge, September 15, 1860.

Dear Sir:—

Learning that you are proposing to take photographic views from a balloon, I should be glad to have you try to ascertain in how much less *time* the upper surface of a dense cloud in full sunshine will make a strong negative than the landscape view of the earth requires.

. . . .

Yours truly,
G. P. Bond.

From Dr. C. H. F. Peters to George Bond.

Hamilton College Observatory,
Clinton, N. Y., April 15, 1861.

My Dear Sir:—

I am in such a state of political excitement to-day that I rather should not write a letter, but I am so much in debt with you that I cannot delay writing any longer. The engraving of the comet is exceedingly beautiful, and if they are all of the same perfection, your work on Donati's Comet will astonish the world in the view of artistic perfection.

. . . .

Corrected, but not quite definitive elements of *Titania* I sent to Bruennow, who now ought to have a little more activity with the *Notices*, since the *Journal* has laid down its arms. We are arranging longitude determinations between here and Ann Arbor for the last week of this month.

. . . .

Without being really ill, I have been continually not well the whole winter; but now, with spring, I begin to dig a little in the garden,* and hope I shall be better. I should like very much to show you our place when the leaves are on the trees. Can you not contrive to make us a visit for a few days, perhaps when you are going to your annual beaver-hunting. Sincerely yours,

C. H. F. PETERS.

P. S.—I must go down to the village and hear what are the latest news—whether it is really true that Fort Sumter treacherously has surrendered.

TO WILLIAM LASSELL FROM GEORGE BOND.

CAMBRIDGE, May 18, 1861.

Dear Sir:—

. . . .

I have lately communicated to the Astronomical Society an account of a spiral, or rather a whirl, character in the light of the great nebula in *Orion*, which I am entirely certain you will recognize with your large telescope; and having once caught the idea, you will fill up the details with greater certainty than I have done.†

. . . .

My time of late has been much occupied in an extensive collection of observations on the great comet of 1858, for which the engravings are now mostly completed; a specimen is inclosed.

Yours very truly,

G. P. BOND.

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*Doctor PETERS was an enthusiastic botanist—skilled in this as in so many things—and his famous roses grew all over the walls of his observatory.

†BOND's expectation was fully carried out in LASSELL's splendid drawing made at Malta with the four-foot reflector.

To E. H. Whitney from George Bond.

Cambridge, July 9, 1861.

Dear Sir:—

In reply to yours of the 8th, I am quite unable to say what would be the effect of a collision with the tail of the comet.

As a mere matter of opinion, I fancy we should be quite unconscious of the event, and know it only from astronomical calculation. Probably contact with the nucleus might be disastrous—somewhat worse than meeting with a cannon ball in full career of a few millons of tons weight. But this is mere conjecture; we only know as a scientific fact, that the tails of comets are attenuated beyond any substance with which we have to do on the earth.

Respectfully yours,

G. P. Bond.

Extracts from a letter to R. C. Carrington from George Bond.

January 3, 1862.

. . . Enclosed are two articles by Mr. Safford. One on the proper motion of *Sirius* in declination, confirming Bessel's hypothesis deduced from the right ascensions alone— viz. that the star circulates about an invisible companion.

The article on the mass of *Neptune* shows that Struve's mass from the satellite, viz. $M = \frac{1}{14491}$, is decidedly discordant. You will find in the *Bulletins* of the St. Petersburg Academy some remarks of O. Struve respecting our observations on the sattellite, from which I found the value $M = \frac{1}{19400}$.

He insinuates that they are defective, but gives no evidence, excepting that they differ systematically from his; and Leverrier seems to have acquiesced by accepting his mass. Mr. Safford's result is $M=\frac{1}{20039}$, independently computed from the perturbations of *Uranus*, and satisfying them in a way which settles the question decisively against STRUVE.*

FROM DR. C. H. F. PETERS TO GEORGE BOND.

HAMILTON COLLEGE OBSERVATORY,
CLINTON, N. Y., 1862, January 13.

Dear Sir:—

. . . .

It is long since anything has struck me so much as your handsome construction of the *catenary* shape of DONATI'S Comet, by which fact you have laid a firmer basis—the first firm basis for the theories of comet tails. It is curious that the nucleus occupies a kind of focus in the catenary—though this curve has no focus. . . .

It seems, however, that the catenary represents the *level* surface of the forces emanating from the nucleus and from the sun, and thus must give the law of these forces, which is expressed in the distances of the nucleus from the surface.

We have to bear in mind, however, that the curve only represents the outline of the nebulous envelope. How are now the famous drawings made at the Dudley Observatory represented by your catenary?

From BRUENNOW I have not heard for about two months. A number of the *Notices*, arrived Saturday night, shows, however, that he is alive. Why the *Notices* do not appear quicker, I do not understand. He has had communications from me since October, enough almost for a whole number. The end of the solar eclipse, on the 31st of December,

* The mass of *Neptune*, from modern observations, is $\frac{1}{19380}$.

was not observed here for clouds. I got a chance, however, before the end to see the sun eclipsed, and was particularly surprised to find the moon to be surrounded by a yellow-brown fringe of two or three seconds in width. This was not visible on the (inner) edge of the sun.

I used the polarizing eyepiece, which shows the sun perfectly white. Yours very truly,

C. H. F. Peters.

To Professor E. Loomis from George Bond.

Observatory of Harvard College,
Cambridge, February 11, 1862.

Dear Sir:—

I inclose for insertion in [Silliman's] *Journal* a notice of the discovery of the new asteroid and observations upon the companion of *Sirius*, lately detected by Mr. Clark with his 18½-inch object glass.

. . . .

The discovery of a companion of *Sirius* so soon after the publication of Mr. Safford's investigations on its motion, which completed the evidence of the existence of an (hitherto) *invisible* companion, is quite remarkable. I had before sent you a copy of his article. Yours truly,

G. P. Bond.

From George Bond to M. Otto von Struve.

Observatory of Harvard College,
Cambridge, Mass., U. S. A., October 28, 1862.

Dear Sir:—

. . . [Acknowledgments of publications, etc.] For these very valuable donations I desire to offer my best thanks to the astronomers, and especially to your venerable father, to whom we have in past years been greatly indebted for similar favors.

. . . .

The account of the Poulkova observations upon the great comet of 1858 I have studied with great interest. A truly admirable collection, honorable to the renowned institution from which it has emanated, and to the astronomers engaged in its production. It would have been well for cometary science if something of the same skill and industry had been more generally exhibited in other observatories.

With my best respects to your father.

Respectfully,
G. P. BOND.

FROM DR. C. H. F. PETERS TO GEORGE BOND.

HAMILTON COLLEGE OBSERVATORY,
CLINTON, N. Y., 1862, November 20.

My Dear Sir:—

It has become my sorrowful duty to announce to you that I have committed the blunder to increase the number of the already too numerous asteroids by one more.* I saw it first on the 12th inst., but clouds prevented anything more than rough drawing into the chart. On the 15th, however, I obtained the following good positions.

. . . .

. . . .

Yours very truly,
C. H. F. PETERS.

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* Doctor PETERS is the discoverer of no less than forty-eight of these small planets.

TRANSLATION OF A LETTER FROM PROFESSOR GALLE TO GEORGE BOND.

BRESLAU, November 25, 1862.

Honored Sir:—

. . . .

I can no longer delay expressing in a few words my pleasure and my most sincere thanks for such a valuable gift. At the first hasty glance at the work I was surprised in an unusual degree at the extraordinary beauty of the drawings of the comet, which are marked by a truthfulness to nature which I, for my part, have hitherto sought for in vain in the most celebrated works upon such subjects. The tone of coloring, the gradations of brightness, the drawings of the details of the head of the comet, all give in the most faithful manner the appearance and the peculiar impression which these wonderful heavenly bodies make upon an unprejudiced mind through a good telescope. Science is enriched by the addition of these monographs to your work, and therefore our thanks are due to you, first of all, for the excellence attained in the drawings, then to the artist for their execution, and indeed to all who have enabled you to publish the book in such perfection. In fact, I think that the manner in which the drawings have been executed could hardly be surpassed, and must serve as a model for all similar undertakings. Figures of such a character form so much more reliable a groundwork for a theory of these heavenly bodies, when, like these, they give the impression of perfect freedom from bias in favor of any theory.

I have perhaps already been too prolix in my discussion, and therefore must refrain for the present from enlarging upon the text, although there also is a rich and valuable store to repay closer study.

. . . .

With the highest respect,

Yours truly,

J. G. GALLE.

TO DR. C. H. F. PETERS FROM GEORGE BOND.

CAMBRIDGE, January 7, 1863.

Dear Sir:—

. . . .

What you say of the financial prospects with which you begin the new year, nearly completes the list of the twenty-five observatories *started* (not *founded*) within the past twenty years in the United States and left to die of want. Now if we except the National Observatory, no doubt we have here at Cambridge the one best provided for among them all. Yet I can say, from bitter experience, that the charge of it has been from the beginning a perpetual vexation of spirit, for the want of proper means of support.

I am weary to death of this new, original and undeniably American idea of cultivating science by withholding all the aid which the rest of the world has hitherto thought requisite, and then haunting the unhappy astronomer with a dismal ghost of popular reputation and newspaper notoriety.

Excepting that in this community there are those who will do what the State neglects, out of pure largeness of views, we should long ago have made one on the list of failures, and have added another warning against the repetition of the folly of a new observatory.

Truly yours,

G. P. BOND.

FROM DR. C. H. F. PETERS TO GEORGE BOND.

HAMILTON COLLEGE OBSERVATORY,
CLINTON, N. Y., 1863, February 1.

Dear Sir:—

. . . .

In the last week I have been . . . working my solar spot observations out of the rough. Of 7000 positions,* nearly half are still on the chronograph sheets. This makes, as each position has been observed four times, together with accessory measures of dimensions, groups, etc., about 16,000 chronograph readings yet to be made. I must try to overcome this physical impossibility until the month of April, when my year is out, and I am going to take leave of absence, the trustees not being willing to pay salary. Lately for a day I was in Albany to speak with a lawyer about the payment of my last year's salary. The trustees here, too, will find that there are "fighting" astronomers. (These last things *entre nous*.)

Yours very truly, C. H. F. PETERS.

FROM DIRECTOR OTTO STRUVE TO GEORGE BOND.

(TRANSLATION.)

POULKOVA, March 3, 1863.

Very Honored Colleague:—

Severe labor and illness consequent thereon are the causes of my not having answered your esteemed letter of the 28th October, and expressed at the same time my cordial thanks for the

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* These and other extensive series of observations were left unpublished at Dr. PETERS' death. It is greatly to be desired that those who have his manuscripts in charge will arrange to have them edited and printed as a tribute to his memory and on account of their intrinsic value.

valuable collection of your publications which shortly followed that letter.

. . . .

In the collection your great work, *Account of the Great Comet of 1858*, has naturally attracted the chief attention. It is incontestably the finest monograph of a comet that astronomical literature can show, and its contents so carefully, so circumspectly, elaborated will certainly contribute essentially to establish our views upon the physical constitution of comets in general, and of the one here treated of in particular. I was especially struck with the analogy between your observations and mine as to the comprehension of the phenomena of the head of the comet. In November last I had the honor of sending to you a copy of my paper on the nebula of *Orion* by the American Consul, Mr. TAYLOR, with the request that he would transmit it to you as quickly as possible, as I believed that it would be particularly interesting to you. I was very anxious to obtain your opinion upon it, as you are the only person beside myself who has studied the subject so thoroughly as to be able to form a competent judgment upon it.

. . . .

The similar optical power of the telescopes used by us has already repeatedly been the cause of our laboring upon the same objects, and naturally we have either quickly opposed or confirmed each other more or less in our results. If God grants us the power of future work, this will still be the case, and I believe that science will be the gainer by it so long as we keep her advancement sincerely in view, and do not allow jealousy to disturb our judgment. That I have followed this fundamental principle you will readily perceive in both papers, the transmission of which I hereby announce to you. In the first I have indeed been obliged to express a rather severe criticism upon the catalogue of the stars in the nebula of *Orion*, published by your late father, and also in the other, I have not altogether agreed with the results given in your otherwise so distinguished work upon the *Rings of Saturn*, but in neither have I withheld my recognition of what was really deserving. In like manner, you also in your latest work upon the comet have extracted what was good in our observations, and I beg of you to accept my hearty thanks for these indications of good will.

This subject reminds me, also, of the discrepancy in our results upon the mass of *Neptune*. It does certainly appear at present as if Mr. SAFFORD's theoretical researches confirmed the value found by you. However, I do not consider this matter finally determined yet, for proof is still wanting that *Neptune alone* has produced those perturbations in the path of *Uranus*, which are explained by the adoption of your estimate.

. . . .

With the highest regard,

Yours most truly,

OTTO STRUVE.

TO DIRECTOR OTTO STRUVE FROM GEORGE BOND.

OBSERVATORY OF HARVARD COLLEGE,
CAMBRIDGE, Mass., U. S., America.
July 15, 1863.

Dear Sir:—

The acknowledgment of your interesting letter of March 3d, which reached me in due course, has been delayed longer than I could have wished, as indeed has been too much the case with all my correspondence, by the necessity which I have been of late subjected to, of devoting much time to observatory duties, which have hitherto devolved upon my assistants. Circumstances, chiefly arising from the unhappy civil war which desolates our country, have deprived the observatory of the services of Mr. COOLIDGE, Mr. HALL and Mr. TUTTLE.

The servant of ordinary work has enlisted, and within the past week even the pupil whom we had taken for instruction to supply the place of more skillful assistants, has been drafted into the army. Mr. SAFFORD alone remains at his post; but I hope we shall be able to rescue our new adjunct, and if blessed with returning health myself, I am still confident that we can maintain a respectable activity, more especially as we have the strong confidence of the community, which has been our principal reliance for material support. We have, also, through the liberality of a merchant of Boston, an accession to our funds for publication, which is very acceptable.

Before this letter reaches you, our latest publications, viz. the *Observatory Report* for 1862, and *Annals*, Vol. IV, Part I, ought to have come to your hands. They were transmitted in May last, *via* Leipsic. I regret to say, that the collection of Poulkova publications kindly promised in your letter has not been received. These delays are exceedingly vexatious, and I fear that, as you have sometimes mentioned, the packages have been placed in the hands of our government diplomatic agents; and that the difficulty lies in that quarter since recent regulations have very stringently forbidden the use of the government dispatch-bags for any other than official papers.

I beg to suggest that it might be well to entrust any package designed for our observatory to some one of the large booksellers in St. Petersburg, to be forwarded to London, *via* Leipsic, the package to be addressed to HENRY TOOKE PARKER, 3 Ladbroke

Gardens, Kensington Park, London, care Messrs. Williams and Norgate, booksellers, London. I believe that parcels once in the hands of Doctor Fluegel, of Leipsic, who acts as agent of the Smithsonian Institution of Washington, are safely transmitted; but the delay is often very great. My recent experience, as well as enquiries directed to this subject in England and on the Continent, have led me to request Mr. Parker of London to act as a receiving and distributing agent of the observatory, and arrangements have been made by which all parcels sent to him from the Continent will be forwarded to us. He has assured me that he can avail himself of the extensive business connections of Messrs. Williams and Norgate with the principal continental booksellers, to establish a safe and prompt means of communication between our observatory and those of Europe. He has instructions to pay all necessary charges, and will, I believe, do everything in his power to secure the objects in view.

Among the Poulkova publications which you mention as having been sent, but which have not come to hand are, the notice of the observations upon *Saturn* in 1862, the Weisse catalogue, the notice of observations for the parallax of *Mars*, and the memoir on the nebula of *Orion*. Should you not shortly receive from me an acknowledgment of their arrival, I fear we must have to conclude that they have been lost on the way.

With the memoir on the nebula of *Orion* I supplied myself while on a short tour in Europe, from which I have just returned. I have as yet been able to give to it only a hasty perusal, which has given me the impression that in the references to my father's observations and catalogue you have expressed a criticism, severe, perhaps, but in its tone quite altered from that which characterizes the passages upon the same subject published in 1857 in the *Monthly Notices* of the Royal Astronomical Society. I am certain that you will not regret this when you come to know all the circumstances of the case. It will not be possible for me, within the limits of a letter, to enter upon explanations, which can, indeed, only be properly done when the original observations are published in detail. This I propose shortly to accomplish, if health is granted to me, for I am persuaded that they have an extent, and have been conducted with an amount of care and labor which do not at all appear in the very imperfect statement of results presented in the memoir. My chief difficulty is to comprehend how the numbers published in the catalogue came to be deduced as the results of the measurements actually made. I have not yet succeeded in finding the manuscripts of the reductions. I think, however, that all the observations are acceptable, and that the greater part have never been reduced, or even alluded to in the memoir.

In regard to the variability of the nebula, I have not yet made such a comparison of our observations at different epochs as the subject would require.

The disturbing influence of atmospheric changes renders this a most perplexing question. I can speak with more confidence of the variable character of several of the stars in its neighborhood, or immersed in its light, but even here the influence of the masses of nebulosity is such as to make every prevention necessary to avoid deception. Still, I cannot avoid concluding from my own comparisons alone, and quite independently of your results, that there are a sufficient number of well-established instances of variability to awaken very special interest.

I look forward with impatience to the time when at least a partial release from duties which now require all my attention will leave me at leisure to take up the discussion of our observations upon the nebula, preparatory to their publication. In reference to Mr. SAFFORD's discussion of the mass of *Neptune* from the perturbations of *Uranus*, I am sorry to say that he has been obliged for the present to defer the publication of the details of the investigation, because our reduced force at the observatory has made its current work press more heavily on those remaining, and left no time for excursions beyond its strict routine.

When in Europe, I was told by Professor ARGELANDER (whom I saw only a week or two after you left Bonn) that you had of late suffered from ill-health, and your letter also intimates the same. I earnestly hope that this may prove only a temporary drawback to your activity.

With my best regards to your father,
I am with much respect,
Very truly yours,
G. P. BOND.

FROM PROF. J. CLERK-MAXWELL TO GEORGE BOND.

GLENLAIR HOUSE, DALBEATTIE, SCOTLAND,
1863, August 25.

Dear Sir:—

. . . .

I shall study what you say about *Saturn* in your letter when I see your drawings and observations. I have no doubt that the time is coming when we shall know more about the heavenly bodies than that they attract each other from a distance. In *Saturn's* ring we certainly have a very wonderful object to examine, and when we come to understand it we shall certainly know more mechanics than we do now.

Your observations of comets' tails go far to render them legitimate subjects of speculation, and I think that when we have mastered the theory of these tails we shall know more about what the heavens are made of. I think the heavenly spaces are by no means empty, since, as THOMSON has shown, a cubic mile of sunlight, even at the earth's distance, is worth, mechanically, 12,050 foot-pounds; and a cubic foot of space near the sun can contain energy equal to .0038 foot-pound, *at least.* This is under ordinary circumstances, and gives an estimate of the amount of strain which the medium has been for ages subjected to, without in any way giving way. But we have no reason to believe that if the sun's heat were increased 1000-fold, the medium would be unable to transmit it, or would break down under the forces applied. We have therefore no knowledge of the ultimate strength of the heavenly medium; but it is well able to do all that is required of it, whether we give it nothing to do but to transmit light and heat, or whether we make it the machinery of magnetism and electricity also, and at last assign gravitation itself to its power.

If we could understand how the pressure of a dense body could produce a linear pressure radiating out in straight lines from the body, and keep up this kind of pressure continually, then gravitation would be explained on mechanical principles, and the attraction of two bodies would be the consequence of the repulsive action of the lines of pressure in the medium.

For instance, in the case of a body [P] at a distance from the sun [S], the equation to the lines of force would be [diagram omitted]: —

$$P \cos \theta + r^2 \sin^2 \theta = C.$$

Where r is the distance from P, and θ the angle which r makes with PS.

There are two sets of lines separated by the surface of revolution whose equation is got by making $C=P.\ r^2=\frac{a^2}{1+\cos\theta}$. This surface has the general shape of a paraboloid of revolution, but suggests the appearance of a comet's tail, being more like a catenary than a parabola near the head. Is there anything about a comet to render its lines of force visible, and not those of a planet which are stronger? I think that visible lines of gravitating force are extremely improbable, but I never saw anything so like them as some tails of comets. What HERSCHEL says about the repulsive action of the sun leaves unexplained the fact that the motion of the nucleus is that of a body gravitating toward the sun with a force neither more nor less than that of ordinary matter. If there were at any time in the comet matter which was not gravitating, or not gravitating to the same extent as earthly matter, then the path of the comet would be less curved to the sun than if it were made of ordinary matter, and therefore calculations depending upon the common value of the sun's attractive power would not give the true path of comets.

I have nothing yet to send you, but we are making a report on electrical measurements for the British Association which I will send you when I get copies, and if you will inform me of any electrical men in America, I will bring forward their claim to have copies of the *Standard Coil of Electrical Resistance.* We have hopes of producing coils next winter, the resistance of which is known to within a small fraction in electromagnetic units. Such coils may be employed in measuring electromotive forces, in determining the mechanical equivalent of heat, and in other researches. The present measures of resistance in absolute units vary by six or seven per cent., but I think we are already safe within one-half of one per cent., and I see how to make determinations quite as exact as we can determine the size of our coil in meters.

In the course of our work we have had to obtain a constant velocity of rotation. This was secured by means of a governor invented by Mr. FLEEMING JENKIN; but we propose to make a new governor, combining the principles of Professor W. THOMSON and Mr. JENKIN; we hope to get good results, comparable with clockwork. I have been studying the mathematical principles of governors, and I have been able to detect the sources of irregularities in the motion, and I hope to correct them. We mean to expose the new governor to severe tests by sudden variations of driving power, and if we find it answer I hope it will be taken into consideration in devising moving power for large equatorials. The dynamics of governors is exceedingly interesting, on account of the number of conditions which may be introduced by various arrangements of the machinery, and the different and sometimes opposite effects of these on the stability of the motion.

I am exceedingly obliged to you for your kindness in sending the books. I hope to be able to say so again when I have read the part about *Saturn.* I think the visibility of the ring under oblique sunshine shows that its surface is very rough, the roughness not being like that of paper or sandstone, but like that of a wilderness of sharp rocks, so that we, being on the same side as the sun, see nearly every spot of sunshine, while most of the shadows are hid by their respective objects. ARAGO's test of the solidity of a heavenly body by polarized light supposes the solid body to be as smooth as a rough bar of iron, if not actually polished, whereas the smoothest part of our earth is a paved street, and even the sea is generally too rough to polarize much light.

With much respect, yours truly,

J. CLERK-MAXWELL.

FROM PROFESSOR SCHOENFELD TO GEORGE BOND.
(TRANSLATION).

MANNHEIM, 1863, December 16.

Dear Sir:—

The first occasion for these lines is the expression of my thanks for your last letter and its enclosure of [the places of] new nebulæ, which was very welcome. I shall, it is true, be able to observe only a part of them with my telescope. . . . Please accept, therefore, my heartiest thanks, and the assurance that I should hardly have ventured the request for these positions had I been able to estimate the length of the list.

Your opinion, that it is probable that the nebulæ as a class are not separated from the fixed stars by relatively great distances, I agree with entirely.

Above all else, the manifold nature of the Magellanic clouds—in which single and double stars, star clusters of every degree of condensation and nebulæ of every degree of resolvability are found commingled—seems to me entirely irreconcilable with the contrary view; not to speak of such remarkable combinations as we find in *h* (399) and other nebulæ. I think, indeed, that there would not be so much opposition to the theory that nebulæ and fixed stars together constitute a single system, if the doctrine of the development of nebulæ into perfect stars, which HERSCHEL connected with it, had excited less opposition. Nevertheless, it seems to me that the two theories can be considered independently; the latter I cannot fully understand, while the former seems very plausible.

The successive parts of my observations I shall, of course, send to you as well as to the observatory; but my hope to be able to complete the collection of the material for the second section this winter seems, unfortunately, unlikely to be realized, as the summer and autumn have been very unfavorable.

. . . As to the newly-established *Astronomische Gesellschaft*, it seems to meet with the approval necessary to increase its resources. Up to the present time it has ninety-four members, a number of whom are not astronomers. Many of the more influential older German astronomers seem, however, to hold themselves aloof. For instance, B. PETERS, HANSEN and ENCKE have not joined. (ENCKE has, moreover, resigned the directorship of the Berlin Observatory and has retired.) On the other hand, among the members, in addition to those already known to you, are GERLING, SCHWERD, MAEDLER, LITTROW, SEIDEL, BRUENNOW and almost all the younger German astronomers. Besides, as you will have noticed in the constitution, the membership is not limited to Germany. We count, as members, nearly all the Italian astronomers (except only PLANA, DONATI and SECCHI, I think), several Swedes, a few Frenchmen (not well known to me otherwise), WOLF and PLANTAMOUR among the Swiss, and among the English, Professor ADAMS, besides Captain OOM in Lisbon. In fact, the society transcends the boundaries of Europe — Doctor MONTY in Tiflis and Doctor NEWMANN in Peking (astronomer of the Russian society in that place) are members. In the name of the society, I express to you my heartiest thanks for the interest with which you regard it. . . . As the society has now extended itself beyond the limits of Europe, I hope you will not consider it impertinent of me to remark that, if the great distance does not deter you, there is no obstacle on our part to your becoming an active member. On the contrary, I am convinced that not only I, but all other members, would consider it an honor to welcome you as a fellow-member. It is true, I feel the society cannot offer you much, aside from its publications and reports, as you will [not] have much opportunity to take part in its meetings.

The consciousness that you are supporting a good cause may, however, be a motive for joining. Our science is surely in no sense national, but rather cosmopolitan. In case you should be inclined to join us, let me add that, until the close of this year, the mere statement is sufficient. I have postponed my letter so long, however, that little of the year is left. But if you send the statement a few days later, under date of December 31, 1863, it will answer; and if not, the admission will, of course, follow, through the unanimous vote of the directors. I beg you, therefore, to overlook my freedom in this matter, and to give me the pleasure of an early and favorable reply.

My wife and I never think of your visit with us without pleasure, and I am sincerely glad to have become personally acquainted with you. My wife requests me to send her best regards to yourself and to your daughters. I am,

Yours most sincerely,
E. SCHOENFELD.

FROM HON. WILLIAM MITCHELL* TO GEORGE BOND.

LYNN, January 26, 1864.

I enclose, my dear fellow, the autograph of a distinguished citizen of Boston, the ex-president of the college and a member of the Observatory Committee.† It was offered as an addition to the

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*A member of the Board of Visitors 1849-65, chairman of the board 1857-65; member of the Board of Overseers 1857-65. He was a Quaker, an amateur astronomer of ability, and the father of MARIA MITCHELL.

†The member referred to is ex-president EDWARD EVERETT.

report which I had prepared, and it was unanimously accepted. The remarks which he made at length in its support were of a most flattering character, and they met the hearty response of every member of the board.

Very truly and ever thine,
WM. MITCHELL.

"The committee, in conclusion, beg here to perform a grateful duty in bearing testimony to the diligence, fidelity and success with which the Director of the Observatory continues to fill his arduous and responsible duties. It affords them much satisfaction to know that his rank among the most distinguished living observers is fully recognized by the most eminent astronomers and scientific bodies of Europe."

FROM GEORGE BOND TO DR. C. H. F. PETERS.

CAMBRIDGE, July 2, 1864.

My Dear Sir:—

It was a pleasure to us all to recognize again your handwriting—not the least to the little girls, who insist that I should write to ask you to make a visit to Cambridge, in which I cordially join. We should have notes to compare, and you must have collected much information which would be of mutual interest. . . .

Mr. ROGERS,* of Alfred Centre, who was formerly a pupil in astronomy here, has been staying at the observatory, for the sake of study and practice, for a few months past. He has the true zeal and entire honesty of character, which I must say does seem to be a trait not over-prominent among American

*WILLIAM A. ROGERS, for many years assistant Harvard College Observatory and Professor of Colby University.

savants. As a case in point, what do you think of——— of——— publishing that article on———, in *Silliman's Journal* for March, 1863, as an original method? When———, in the same journal for July, 1864, reproduces the same, "faithfully" transcribed from Doctor BRUENNOW'S lectures at Ann Arbor in 1858, at which——— was one of the auditors. You will see that——— has appropriated from BRUENNOW, without a syllable of acknowledgment, the whole substance of the method, embellishing it with a little fancy work of his own, which amounts to nothing but a flimsy concealment of the outrage.

You have seen, of course, Dr. TAPPAN'S statement of affairs at Ann Arbor, which led to his and BRUENNOW'S resignation.

. . . .

Believe me very truly yours,
G. P. BOND.

[The blanks in the printed letter were filled in the original.]

FROM GEORGE BOND TO DR. HENRY DRAPER.

OBSERVATORY OF HARVARD COLLEGE,
CAMBRIDGE, Mass., November 15, 1864.

Dear Sir:—

Through the kindness of Mr. FOLSOM, I have received, in perfect condition, the magnificent photograph of the moon, with the accompanying memoir, which you have presented to the observatory. Please accept my best thanks for this fine specimen of your successful labors in celestial photography. You seem to have surrounded yourself with advantages quite unrivaled. Chief among them, I should reckon that of joining to your own knowledge of the theory of the chemical process involved, the fruit of your father's long experience and profound researches.

Will you allow me to suggest to you the great importance of attaining such a degree of sensitiveness in the plates that they will furnish, at moderate exposures, images of telescopic stars? There is no method known of determining the distance and angle of position of double stars, which is so exact as that by photography; but our experiments at this observatory have been arrested at about the sixth — seventh — magnitude. I believe that we shall never know how much may be accomplished in astronomy by this beautiful art, until some one imbued like yourself with zeal and the knowledge which comes only from practical experience, shall transfer his apparatus to more favored skies, whose atmospheric disturbances shall be less annoying than here.

In our climate the case is absolutely hopeless. Through seventeen years, during which I have constantly used a large telescope, it has never afforded me a glimpse of the image of a celestial object not sensibly deteriorated by undulations in our atmosphere. Respectfully yours,

G. P. Bond.

From Dr. Henry Draper to George Bond.

University of New York, November 21, 1864.

Dear Sir: —

I have just received your note, and am much pleased to find that the photograph has reached you in good condition. I shall write to thank Mr. Folsom for the trouble he has taken, as soon as I can procure his address.

The remarks you make, concerning the atmosphere in which we have to work here, coincide with those that I have been compelled to make in some recent papers for the London journals. I do not see the way to doing much better at this level.

. . . It has seemed to me probable that the best place for celestial photography would be somewhere on the west coast of South America—near the equator, for instance,—in the neigborhood of Quito, and at a considerable altitude above the sea. In this locality, the nights on which it has been worth while to work at the moon in the past two years have only been three or four in number, and even on these, a bright star would exhibit some flickering motion. I have never seen an occasion on which the full optical power of the 15½-inch mirror could be realized, and images obtained equal to those seen when only a short column of air is used, as in testing the figure at the centre of curvature.

The cost, however, of an expedition to a distance is greater than an amateur could meet, to say nothing of the time required. I trust that we may soon see the capabilities of photography applied to astronomy thoroughly investigated.

My attention has already been attracted to stellar photography by the excellent results you obtained and published. I have made and mounted, equatorially, a nine-inch mirror for studying that branch of the subject, but have not as yet had a fair opportunity of using it.

With many thanks for the kind expressions contained in your letter, I remain,

Yours truly,

HENRY DRAPER.

To Professor Asaph Hall from George Bond.

January 7, 1865.

. . . .

My disease makes progress, and leaves me little hope of putting the materials of my work on *Orion* —to which I had devoted so much labor—into condition such that another could prepare them for press. In truth, I am becoming resigned to the idea that most of it is destined to oblivion.

I had planned to accomplish something considerable, and this is the end. "It is not in man that walketh to direct his steps."

Very truly yours,
G. P. Bond.

From Dr. C. H. F. Peters to George Bond.

Hamilton College Observatory,
February 1, 1865.

My Dear Sir:—

. . . .

A few days ago I learned that your health is very bad. This has put me into the greatest anxiety, for you may surely believe there are few who appreciate and esteem you as much as I do. You will pardon me, therefore, if I suggest and urge upon you the necessity of dwelling for some time in a warmer climate, abstaining for a little time from such hard working as you do, and living entirely for regaining your physical health. I beg you, for the sake of science, for your children, for your friends. Go as soon as possible, at least before the poisonous thaws of spring set in.

. . . .

I beg you, once more, consider my suggestion, which flows from sincere friendship.

Yours very truly,

C. H. F. PETERS.

FROM THE FOREIGN SECRETARY OF THE ROYAL ASTRONOMICAL SOCIETY TO PROFESSOR GEORGE BOND.

ROYAL ASTRONOMICAL SOCIETY,
SOMERSET HOUSE, LONDON, 10 February, 1865.*

My Dear Sir:—

I have the pleasing duty of forwarding to you the gold medal of the Royal Astronomical Society, which has been awarded to you this day, "for your work on the Comet of DONATI, and for your other astronomical labors."

I profit by this opportunity to assure you of the high esteem in which you are held by the Fellows of the Royal Astronomical Society, and of the interest which they take in your scientific pursuits. Be assured of my respect and kind regard, and believe me, my dear sir, Yours most truly,

R. H. MANNERS,
Vice-Admiral, Foreign Secretary of R. A. S.

P. S.—I will take care to send you the President's address to the society, on the reasons for awarding the medal, as soon as it is printed.

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* BOND died on the 17th of February, 1865, before this letter reached him. Private advices from his friends in London, Doctor DE LA RUE and others, had informed him of the intended award, however.

CHAPTER V

THE SCIENTIFIC WORK OF WILLIAM AND GEORGE BOND*

IN order to understand the services which were rendered by the BONDS to the science of their country, it is necessary to picture to ourselves the conditions which existed in America during the first portions of the present century, and to glance at the work of individuals and of the earliest institutions and centres of influence. It requires an individual to create and organize an institution. It is impossible, even in the briefest sketch, not to emphasize the debt of American science and learning to the intelligent interest and patronage of our early Presidents—WASHINGTON, JOHN ADAMS, JEFFERSON, MADISON, MONROE, JOHN QUINCY ADAMS. The powerful impetus given by them and through them has shaped the liberal policy of our governments, National and State, towards education and towards science. Sir LYON PLAYFAIR, in his address to the British Association for the Advance-

*The first pages of this chapter were printed in *Science* for June 18, 1897, under the title "The Beginnings of American Astronomy."

ment of Science (1885), has recognized this influence in the truest and most graceful way. He said:—

"In the United Kingdom we are just beginning to understand the wisdom of WASHINGTON'S Farewell Address to his Countrymen (1796), when he said: 'Promote, as an object of primary importance, institutions for the increase and diffusion of knowledge; in proportion as the structure of a government gives force to public opinion, it is essential that public opinion should be enlightened.'"

Until the Revolution (1776) American science was but English science transplanted, and it looked to the Royal Society of London as its censor and patron. WINTHROP, FRANKLIN and RITTENHOUSE were, more or less, English astronomers. FRANKLIN was the sturdiest American of the three. As early as 1743 he suggested the formation of the American Philosophical Society of Philadelphia. JOHN ADAMS founded the American Academy of Arts and Sciences in Boston in 1780. These two societies, together with Harvard College (founded in 1636), Yale College (1701), the University of Virginia (founded by JEFFERSON in 1825) and the U. S. Military Academy at West Point (1801) were the chief foci from which the light of learning spread.

The leading school of pure science was the Military Academy at West Point, and it continued to hold this place till the Civil War (1861).

From its corps of professors and students it gave two chiefs to the United States Coast Survey; and the Army, particularly the Corps of Engineers, provided many observers to that scientific establishment, besides furnishing a large number of professors and teachers of science to the colleges of the country.* The observatory of the Academy was founded by BARTLETT in 1841, and considerable work was done there—only a small part of which is published. The Coast Survey was a school of astronomical practice for army officers, and their experience was utilized in the numerous boundary surveys of 1830–50. Col. J. D. GRAHAM, of the Engineers, for example, was astronomer of the survey of the boundary between Texas and the United States in 1839–40; commissioner of the northeast boundary survey, 1840–43; astronomer of the northwest boundary survey, 1843–47; of the boundary survey between the United States and Canada, 1848–50; of the survey of the boundary between Pennsylvania and Virginia, 1849–50; of the boundary survey between Mexico and the United States, 1850–51. The names of BONNEVILLE, TALCOTT, CRAM, EMORY and other army officers are familiar

*Civil occupations of graduates of the United States Military Academy, 1802–78: presidents of universities and colleges, 35; principals of academies and schools, 27; regents and chancellors of educational institutions, 11; professors and teachers, 119, etc., etc.

in this connection, and their work was generally of a high order. It was in such service that TALCOTT invented, or re-invented, the zenith telescope, now universally employed for all delicate determinations of latitude. The mechanical tact of Americans has served astronomy well. The sextant was invented by THOMAS GODFRAY, of Philadelphia, in 1730, a year before HADLEY brought forward his proposal for such an instrument.* The chronograph of the BONDS, the zenith telescope of TALCOTT, the break-circuit chronometer of WINLOCK are universally used. The diffraction gratings of RUTHERFURD were the best to be had in the world till they were replaced by those of ROWLAND. The use of a telescope as a collimator was first proposed by RITTENHOUSE. The pioneer opticians of the United States were HOLCOMB (1826), FITZ (1846 or earlier), CLARK (1845), SPENCER (1851). Only the CLARKS have a world-wide reputation. WUERDEMANN, instrument-maker to the United States Coast Survey (1834), had a decided influence on observers and instrument-makers throughout the United States, as he introduced among us extreme German

* In 1700 Sir ISAAC NEWTON sent drawings and descriptions of a reflecting sextant to HALLEY for his advice. At HALLEY's death these were found among his papers. HADLEY's device (1731) was undoubtedly derived from NEWTON's manuscript. The Royal Society of London granted £200 to GODFRAY for his reflecting quadrant.

methods where extreme English methods had formerly prevailed.

The system of rectangular land surveys, which proved so convenient for the public lands east of the Rocky mountains, was devised and executed by MANSFIELD, a graduate of the Military Academy.

The list of army officers who became distinguished in civil life as professors in the colleges of the country, is a very large one. COURTENAY (class of 1821 at West Point) was professor of mathematics at the University of Pennsylvania, 1834–36, and at the University of Virginia 1842–43, and the author of admirable text-books. NORTON (class of 1831) became professor at New Haven, and wrote a very useful text-book of astronomy in 1839; and the list could be much extended. The excellent training in mathematics at West Point (chiefly in French methods) early made itself felt throughout the whole country. The mathematical text-books of PEIRCE, of Harvard, and of CHAUVENET, of Annapolis, brought the latest learning of Europe to American students. MITCHEL (class of 1829 at West Point) was the only graduate who became a professional astronomer (1842–61). His direct service to practical observing astronomy is small, but his lectures (1842–48), the conduct of the Cincinnati Observatory (1845–59), and his publication of the *Sidereal Messenger* (1846–48), together with his popular books, excited an intense and wide-spread public interest in the science, and indirectly led to the foundation of many observatories. He was early concerned in the matter of utilizing the electric current for longitude determinations, and his apparatus was only displaced because of the superior excellence of the chronograph devised by the BONDS. His work was done, under immense disadvantages, in a new community (Ohio), but the endowment of astronomical research in America owes a large debt to his energy and efforts.

The Navy and the United States Naval Academy (founded by BANCROFT, 1845, at the suggestion of CHAUVENET), were very active in astronomical work. CHAUVENET (Yale College, 1840) published a textbook of trigonometry, in 1850, which had an important share in directing attention to rigid, elegant and general methods of research. His *Astronomy* (1863) is a handbook for all students. WALKER, GILLISS, COFFIN, HUBBARD, FERGUSON, KEITH, YARNALL, WINLOCK, MAURY, WILKES, were all connected with the Navy, more or less intimately. WALKER's career was especially brilliant. He graduated at Harvard College in 1825, and established the observatory of the Philadelphia High School in 1840. He was the leading spirit in the Naval Observatory at Washington, 1845–47, and introduced modern methods into its practice at the beginning. From the observatory he went to the Coast Survey to take charge of its longitude operations, and he continued to direct and expand this department until his death, in 1853. To him, more than to any single person, is due the idea of the telegraphic method ("the American method") of determining longitude. His able assistant in this work was GOULD, who succeeded to the charge of it in 1853. WALKER's researches extended to the field of mathematical astronomy, also, and his theory of *Neptune* (then newly discovered) marks an important step forward. His investigations and those of PEIRCE were conducted in concert, and attracted general and deserved attention.

The Coast Survey began its work in 1817, under HASSLER, a professor from West Point, who impressed upon the establishment a thoroughly scientific direction. BACHE, his successor (a grandson of BENJAMIN FRANKLIN), was a graduate of West Point in the class of 1825, and took charge of the survey in 1843. He is the true father of the institution and gave it its practical efficiency and high standard. He called around him the flower of the army and navy, and was ably seconded by the permanent corps of civilians, assistants of the survey (WALKER, SAXTON, GOULD, DEAN, BLUNT, POURTALES, BOUTELLE, HILGARD, SCHOTT, GOODFELLOW, CUTTS, DAVIDSON, and others).

SILLIMAN'S (and DANA'S) *American Journal of Science* had been founded at New Haven in 1818, and served as a medium of communication among scientific men. A great step forward was made in the establishment of the *Astronomical Journal* by Doctor GOULD, on his return from Europe at the close of 1849.* *Silliman's Journal* was chiefly concerned with the non-mathematical sciences though it has always contained valuable papers on mathematics, astronomy and physics; especially from the observers of Yale College—OLMSTED, HERRICK, BRADLEY, NORTON, MASON, NEWTON, LYMAN and others. In MASON, who died in 1840, at the age of twenty-one, the country lost a practical astronomer of the highest promise.†

GOULD'S *Journal* was an organ devoted to a special science. It not only gave a convenient means of prompt publication, but it immediately quickened research, and helped to enforce standards already formed, and to establish new ones.

The *Astronomical Notices* of BRUENNOW (1858–62) might have been an exceedingly useful journal with an editor who was willing to give more attention to details, but, in spite of BRUENNOW'S charming personality and great ability, it had

* The *Astronomische Nachrichten* had been founded in Altona by SCHUMACHER in 1821.

† See the *International Review*, vol. 10, page 585.

comparatively little influence on the progress of American science.

The translation of the *Mécanique Céleste* of LAPLACE, by NATHANIEL BOWDITCH, the supercargo of a Boston ship (1815–17), marks the beginning of an independent mathematical school in America. The first volume of the translation appeared in 1829. At that time there were not more than two or three persons in the country who could read it critically. The works of the great mathematicians and astronomers of France and Germany — LAPLACE, LAGRANGE, LEGENDRE, OLBERS, GAUSS, W. STRUVE, BESSEL — were almost entirely unknown. BOWDITCH's translation of the *Mécanique Céleste*, and still more, his extended commentary, brought this monumental work to the attention of students and within their grasp. His *Practical Navigator** contained the latest and best methods for determining the position of a ship at sea, expressed in simple rules. American navigators had no superiors in the first half of this century. One of the sister ships to that on which BOWDITCH was supercargo was visited at Genoa by a European astronomer of note (Baron DE ZACH),

* First edition, 1802. "SUMNER's Method in Navigation" (1843) — a very original and valuable contribution from a Boston sea captain — and MAURY's Wind and Current Charts, begun in 1844, are two other notable contributions from a young country to an art as old as commerce.

who found that the latest methods of working lunar distances to determine the longitude, were known to all on board, sailors as well as officers. His bewilderment reached its climax when the navigator called the negro cook from the galley and bade him expound the methods of determining the longitude to the distinguished visitor. On BOWDITCH's own ship there was "a crew of twelve men, every one of whom could take and work a lunar observation as well, for all practical purposes, as Sir ISAAC NEWTON himself." Such crews were only to be found on American ships (where all were cousins, and each had a share in the voyage,) in the palmy days of democracy. These anecdotes may serve as illustrations of the intellectual awakening which came about so soon as our country was relieved from the pressure of the two wars of 1776 and 1812.

An early visitor, Baron HYDE DE NEUVILLE (1805), felt "an unknown something" in the air; "a new wind blowing." This new spirit, born of freedom, entered first into practical life, as was but natural; science felt its impulse next, and, last of all, a literature was born. EMERSON hailed it in 1837 "as the sign of an indestructible instinct. Perhaps the time is already come — he says — when the sluggard intellect of this country will look from under its iron lids and fill the postponed expectation of the world with something better than the exertions of mechanical skill. Our day of dependence, our long apprenticeship to the learning of other lands draws to a close. The millions that around us are rushing into life cannot always be fed with the sere remains of foreign harvests."

BENJAMIN PEIRCE, a graduate of Harvard in the class of 1828, had been concerned with the translation of the *Mécanique Céleste*, and was early familiar with the best mathematical thought of Europe. He became professor in Harvard College in 1833, and after the death of BOWDITCH, in 1838, he was easily the first mathematical astronomer in the country. His instruction was precisely fitted to develop superior intelligences, and this was his prime usefulness. Just such a man was needed at that time. Beside his theoretical researches on the orbits of *Neptune* and *Uranus* and the moon, his studies on the theory of perturbations, and his works on pure mathematics and mechanics, he concerned himself with questions of practical astronomy, although the observations upon which he depended were made by others. He was the consulting astronomer of the *American Ephemeris and Nautical Almanac* from its foundation in 1849, and its plans were shaped by him to an important degree.

His relative, Lieutenant DAVIS, U. S. N. (the translator of GAUSS's *Theoria Motus Corporum Cælestium*

(1857), was placed in charge of the *Ephemeris*, and the members of its staff (RUNKLE, FERREL, WRIGHT, NEWCOMB, WINLOCK and others) most effectively spread its exact methods by example and precept. Professor PEIRCE undertook the calculations relating to the sun, *Mars* and *Uranus* in the early volumes of the *Ephemeris*. As a compliment to her sex, Miss MARIA MITCHELL was charged with those of *Venus; Mercury* was computed by WINLOCK, *Jupiter* by KENDALL, *Saturn* by DOWNES, *Neptune* by SEARS WALKER.

The Smithsonian Institution was founded in 1846, and JOSEPH HENRY was called from Princeton College to direct it. There never was a wiser choice. His term of service (1846–78) was so long that his ideals became firmly fixed within the establishment and were impressed upon his contemporaries and upon a host of younger men. The interests of astronomy were served by the encouragement of original research by subsidies and otherwise, by the purchase of instruments for scientific expeditions, by the free exchange of scientific books between America and Europe, and by the publication of the results of recondite investigations. It is by these and like services that the institution is known and valued among the wide community of scientific men throughout the world. But this enumeration of specific benefits does not convey an adequate idea of the immense influence exercised by the institution on the scientific ideals of the country. It was of the first importance that the beginnings of independent investigations among Americans should be directed towards right ends and by high and unselfish aims. In the formation of a scientific and, as it were, of a moral standard, a few names will ever be remembered among us; and no one will stand higher than that of HENRY. His wise, broad and generous policy, and his high personal ideals were of immense service to his colleagues and to the country. The present volume contains a letter of his (Chapter IV, dated August 26, 1859), which is an illustration of his generous anticipation of scientific needs.

The establishment of a National Observatory in Washington was proposed by JOHN QUINCY ADAMS in 1825; but it was not until 1844 that the United States Naval Observatory was built by Lieutenant GILLISS of the navy, from plans which he had prepared. By what seems to have been an injustice, GILLISS was not appointed to be its first director.* This place fell to Lieutenant M. F. MAURY.

GILLISS had been on detached service for some years, and a rigid construction of the rules required that he should be sent to sea, and not remain to launch the institution which had been built and

* GILLISS was, however, director during the years 1861-65.

equipped by his efforts. The first corps of observers at Washington (1845) contained men of first-class ability — WALKER, HUBBARD, COFFIN. GILLISS's work on Capitol Hill (1838–42) had shown him to be one of the best of observers, as well as one of the most assiduous, and his study and experience in planning and building the observatory had been of great service to him.

To the men just named, with PEIRCE, GOULD and CHAUVENET, and to their coadjutors and pupils, we owe the introduction of the methods of GAUSS, BESSEL and STRUVE into the United States; and it is due to their influence that American astronomy is the child of German, and not of English science. The most natural evolution might seem to have been for Americans to follow the English practice of MASKELYNE and POND; but the break caused by the War of Independence, by the War of 1812, and by the years necessary for our youthful governments to consolidate (1776–1836) allowed our young men of science to make a perfectly unbiased choice of masters. The elder BOND received *his* impetus, however, from British sources during his visit to England in 1815. GILLISS visited France for study (1835) before he took up his duties at Washington. The text-books of WILLIAM BOND and GILLISS were the Astronomies of PEARSON (1824–29) and VINCE (1797–1808). The younger

BOND and his contemporaries, on the other hand, were firmly grounded in the German methods, then, as now, the most philosophical and thorough. It was not until 1850, or later, that it was indispensable for an American astronomer to read the German language, and to make use of the memoirs of BESSEL, ENCKE, STRUVE, and the text-books of SAWITSCH and BRUENNOW.* This general acquaintance with the German language and methods came nearly a generation later in England. The traditions of PIAZZI and ORIANI were brought to America by the Jesuit Fathers of Georgetown College (1844), of whom SECCHI and SESTINI are the best known.

The dates of the foundation of a few observatories, etc., of the United States may be set down here. Those utilized for the observation of the transit of *Venus* in 1769 were temporary stations merely. The first college observatory was that of Chapel Hill, North Carolina (1831); Williams College followed (in 1836); Hudson Observatory, Ohio (1838); the Philadelphia High School (1840); the Dana House Observatory of Harvard College (1840); West Point (1841); the United States Naval Observatory (1844); Georgetown College Observatory (1844); the Cincinnati Observatory (1845); the new Observatory of Harvard College (1846); the private observatory of L. M. RUTHERFORD (1848); the

* BOWDITCH learned German in 1818, at the age of forty-five.

Observatory of Ann Arbor (1854); the Dudley Observatory (1856) and that of Hamilton College (1856).

The foregoing outline will serve to indicate the situation of the astronomy of the country during the first half of the present century. A little attention to the dates will enable the reader to place an individual or an institution on the proper background. It must be constantly kept in mind that the country was very young, and that public interest in astronomical matters was neither educated nor very general. The first business of the elder Bond was to establish an observatory at Harvard College; and we have seen that the Dana House served its purpose for the earlier years (1840–46). It was not long, however, before the final plan was conceived, and by the generous help of the community it was soon executed. In 1846 the present observatory was completed. It was furnished in 1847 with the 15-inch equatorial telescope, at that time the largest that had been made. The Imperial Observatory of Russia (Pulkowa) had one of like dimensions, but these two were by far the most perfect, as well as the largest instruments existing. Other needed instruments were provided and mounted. The minor apparatus was on hand. The observatory was endowed. Important series of observations had already been commenced. All this had been done in a few years by the two BONDS. A complete observatory of the first class had been created. A significant word in regard to the services of the BONDS in this respect is to be found in the *Eloge* of WINLOCK,* written by Professor LOVERING of Harvard, who knew them well. He says: "Their lives, consecrated to astronomy, founded the observatory and won for it the sympathy and support of the community. Affection for them, and respect for their disinterested zeal, inspired the liberal endowments which strengthened its early growth. Because the men were there the institution was born and lived."

The observatory was now in existence. The great telescope was mounted in June, 1847. It remains to show how it was used during the eighteen remaining years of their directorship. The full list of the papers printed by them may be found in the appendices, and it will be interesting to turn to these lists and to point out a few of the most important works.

Nearly every astronomical observation has for its object to determine the *position* of some heavenly body (as a planet) at a given time. By making a second observation of the same sort at a subsequent

* *Biographical Memoirs*, National Academy of Sciences, Vol. I, p. 333.

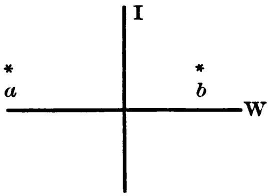
time, the motion of the planet becomes known, and from a sufficient number of such observations, its orbit can be determined. The instrument chiefly used for observations of the sort is a meridian circle, so-called. This is a telescope at right angles to a horizontal axis, having the axis placed exactly in an east and west line. The telescope can revolve, and its central line will trace out the meridian upon the celestial sphere. The angle through which it revolves is measured by a circle graduated into degrees, minutes, etc., fastened to the horizontal axis and turning with it. As the divisions of the circle move past a fixed pointer, the angle is indicated.

Every star passes the meridian once daily in its course from rising to setting, and just at the moment of its passage it can be seen in transit. The observation of a star for position with the meridian circle consists in setting the telescope at the right altitude just before the star transits. It will enter the field of view of the telescope at its east side, and move rapidly across the field and disappear on the west side. The field of view of the telescope has a spider thread fixed horizontally (W in the figure).

a *
b *———————————————W

As the star enters at *a*, for example, the telescope must be gently moved until the star is at *b* and travels along the thread. The circle is now read. Suppose it to indicate 10° 0′ 0″. If a second star subsequently travels along the thread when the circle reads 20° 0′ 0″, then one of these stars is exactly 10° north of the other. The observation has fixed the difference of their altitudes.

Besides the horizontal thread (W) in the eye-piece, there are several vertical threads, one of which (I) is shown in the next figure. What we wish to know is, the exact instant (hour, minute, second and decimal of a second) at which the star crosses the thread I. The method of determining this up to the year 1850 was as follows:—



A clock was placed near the observer, who heard each of its beats (a second apart) distinctly. If the star was on the thread at the exact instant of a clock-beat, the second corresponding to that beat must be written down, as $17^s.0$. The observation was made. In general, the transit will not occur on the exact beat, but at the seventeenth second (for example) the star will be at some point in the field, as *a;* while at the eighteenth beat the star will have crossed the thread and will be seen at *b.*

It has moved over the distance *a b* in a second. The thread is (say) six tenths of the distance *a b* from *a*. Hence the transit occurred at $17^s.6$. Several threads (seven or nine) are observed in this way for every star, and the average of the observed times is taken, so as to obtain a little greater precision; but for the purpose of illustration we may confine our attention to a single thread at I. A certain star crossed it at $1^h\ 0^m\ 17^s.6$, let us say. Some other star crosses it at $1^h\ 32^m\ 20^s.7$. This second star is $32^m\ 3^s.1$ *east* of the first; *east* since it comes later to the thread. By our observations we have shown one star to be 10° north of the other; and their distance east and west to be $32^m\ 3^s.1$.

Starting from any one star—as *Sirius*, for example—we can fix the place of every star in the sky with reference to it. Such, in very brief abstract, is the method of fixing star positions. It consists of two parts, one of which is very simple. Any intelligent lad can be taught in a night to place the horizontal wire so that a star will travel along it. But it requires long practice to estimate, and remember, the relative positions of the star at two successive clock-beats, and to refer these positions of the moving star to the fixed thread without error.*

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* Moreover, every observer is found to have a physiological idiosyncracy in such estimations. Instead of seeing the star at *a*, for example, he will see it a little to the right (or left), so that his observations will differ by a little from those of

It will be obvious that a method of observing which would do away with most of the difficulties of such observations and make the transits as easily observed as the altitudes, would be of the highest value to practical astronomy. It was precisely such a method that was perfected by the BONDS in their "spring-governor," or, as it is called to-day, their *chronograph*. Before giving the history of this invention, we may briefly describe its use without going into details.

Suppose that we have a horizontal cylindrical barrel or drum which is caused, by clockwork, to revolve once in one minute of time. Let us wrap a sheet of paper around the barrel and rest a pen upon the paper. The pen itself rests on a carriage which moves slightly (say the tenth of an inch per minute) lengthwise along the barrel while the latter is turning. Now, if the pen marks, it will trace a smooth spiral line on the paper, round and round, and each complete revolution of the barrel will correspond exactly to one *minute* of time. Let us suppose, further, that the pen is fastened to an

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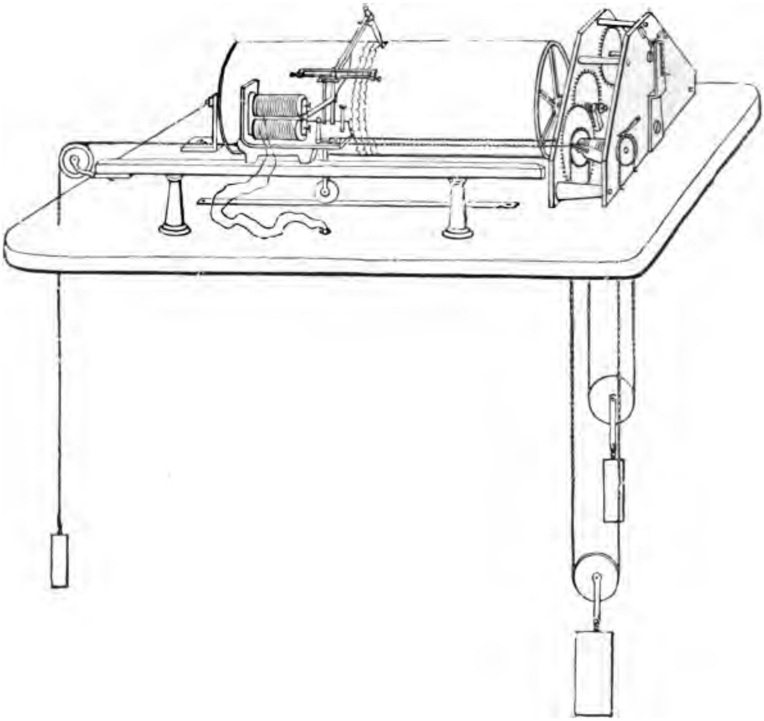
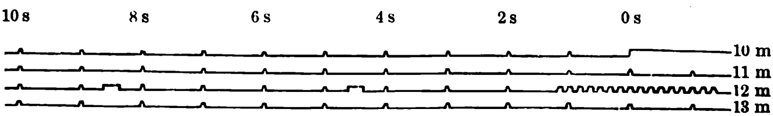
another astronomer. This constant physiological difference of two persons in estimating the same phenomenon is their relative *personal equation*. This term — personal equation — has found its way into literature and into common speech, with a slightly altered meaning. It is generally used to denote an individual's departure from an absolute standard — his total idiosyncracy. In astronomy there are few absolute standards, and the term is employed to denote the idiosyncracy of one observer relative to another, each person being fallible.



THE CHRONOGRAPH INVENTED BY THE BONDS.



PART OF A CHRONOGRAPHIC RECORD.

N. B.—The rattle at 12 m is to call attention to an observation to come. At 4.4 s a star crossed wire I, and at 8.4 s it crossed wire II.

electro-magnet, and that the standard clock is connected to the magnet by a telegraph wire so that the clock can (automatically) *telegraph* its beats to the magnet. At every signal the magnet will move the pen slightly to one side and produce a slight indentation in the (otherwise) smooth spiral. The distance on the paper between any two consecutive indentations is precisely one *second* of time. We have turned time into space, a second into a distance. One more step is required. Let a short telegraph line run from the magnet to a signal-key in the observer's hand. When he presses the key the magnet is affected and the pen moves so as to make a little indentation in the spiral trace. The shape of the indentation corresponding to the observer's signal is a little different from those corresponding to the clock beats. The observer's signal generally falls between two signals from the clock. It is sixty-five hundredths of the distance from the seventeenth to the eighteenth beat, let us say. The observation time is then $17^s.65$. The accompanying cuts will illustrate the description.

Such is the outline of a most valuable invention which has been of priceless value to practical astronomy, and which has countless other applications.*

*For example, the velocity of projectiles is measured by allowing them to break two wire targets, one just in front of the muzzle of the gun, the other 100 yards away. The rupture of each target records a signal on the chronograph. The

Much fatigue is spared by the new method. Observing nights can be made longer. There are no errors of record. The clock, not the astronomer, writes down the time. The record on the chronograph is permanent, and the sheets may be bound, like a book, for future reference. The personal equations are smaller and more constant, also.

Moreover, two clocks may be made to record on the same sheet, and thus compared. One of the clocks may be in New York and the other in San Francisco, and their records will faithfully exhibit their difference of time. But the difference of time is nothing but the difference of *longitude;* and here we have a perfect method of determining positions of places on the earth. Wherever the telegraphic wire runs, we may transmit the signals of a clock. This is the "American" method of determining longitude, first practiced over the line from Baltimore to Washington in 1844, and since then employed in every country of the globe from England to Brazil, from the Cape of Good Hope to the Indies. A slight modification in the apparatus permits the method to be used on ocean cables, and the most distant countries of the world have been united by these bonds.

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distance apart of the signals shows how long the projectile required to move 100 yards. The principle of this method is the same as that of the astronomer's chronograph. The apparatus is more refined, however.

In an official report to the chief of the United States Coast Survey in 1851, Mr. Sears Walker gives the history of the use of the electric current in registering observations, and of the various steps which led to the invention of the chronograph. I have found among the papers of George Bond what I suppose to be the original of Walker's memorandum, in Bond's handwriting, and it is printed below. In a somewhat fuller form it is given in the *Annals* of the Harvard College Observatory, Vol. I, page xxiv.

The following is an abstract of the history of the invention, recently brought into use in America, by which electro-magnetism is introduced as an agent in the determination of differences of terrestrial longitude, and for various astronomical purposes in which the exact noting of time enters as an important element.

On the 9th of June, 1844, Captain Charles Wilkes, U. S. N., made the first experiment for determining longitudes, by means of the electro-telegraph, between Washington and Baltimore, with chronometers rated at each place.

On the 10th of October, 1846, star signals were first exchanged between the Washington Observatory and that of the Central High School of Philadelphia. The outfit of telegraph junction lines and apparatus was made by the Coast Survey. . . .

The experiment was made under the charge of Sears C. Walker, Esq., one of the assistants of the Coast Survey, who, from that time to the present, under an appointment from Professor A. D. Bache, LL. D., superintendent, has had uninterrupted charge of this work. The apparatus used this evening was devised and constructed by JOSEPH SAXTON, Esq. The star signals, or taps on a make-circuit finger key at the instant of the passage of a star over a wire of a transit instrument, were made that night by Lieutenant J. J. ALMY, U. S. N., and were recorded by the ear of Mr. WALKER and Lieutenant J. M. GILLISS, U. S. N., at Washington, and Professor E. O. KENDALL, director of the Philadelphia High School Observatory at Philadelphia. The longitude between the two stations by this night's work agrees within $0^s.2$ with the average of all the work done since.

On the 27th of July, 1847, coincidence of beats of solar and sidereal chronometers were, for the first time, tried between Philadelphia and Jersey City. These coincidences were noted at each place by comparison of a solar and sidereal timekeeper. The circuit of the telegraph line was closed temporarily every ten seconds by the astronomer at one of the stations, and the receiving magnet beats were heard sensibly at the same instant of absolute time at both stations. The date of coincidences of these magnet beats with the stationary clock beats (the one being at solar, the other at sidereal time) were recorded at both stations. This experiment was repeatedly performed that year by Mr. WALKER, assisted at Philadelphia by Professor E. O. KENDALL, director, and at Jersey City by Professor E. LOOMIS.

In July and August, 1848, an extensive series of star signals and clock signals by coincidences were exchanged between the Harvard Observatory at Cambridge, Mass., and the observatory in the garden of the late PETER STUYVESANT in New York City. The work was under the charge of Mr. WALKER. Mr. BOND was the observer at Cambridge, and Professor E. LOOMIS at New York City.

During these experiments Mr. BOND conceived the idea of using an automatic circuit interrupter.*

October 26, 1848, Professor O. M. MITCHELL,† at the suggestion of Mr. WALKER, prepared a circuit interrupter with an ordinary eight-day clock, and used it to graduate the running fillets of paper upon a MORSE register fillet. It was not used in the work with Philadelphia, clouds having prevented work on the 27th, proposed for the purpose. The same mode which Professor MITCHELL used had been proposed by JOSEPH SAXTON, Esq., in 1846, but has not been adopted by Professor BACHE and Mr. WALKER, from apprehension of injury to the performance of the astronomical clock which must be used for the purpose.

October 26, 1848, Dr. J. LOCKE [of Cincinnati] was requested by Mr. WALKER to undertake experiments to obviate some difficulties which it was supposed might result from the deflagration of the clock pallets.

On the 17th of November, 1848, Mr. WALKER extended a junction wire from the Cincinnati telegraph office so as to embrace Doctor LOCKE's clock

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* Such a clock was made by Mr. BOND in February, 1850.

WILLIAM BOND TO HON. WILLIAM MITCHELL.

HARVARD OBSERVATORY,
CAMBRIDGE, February 15, 1850.

My Dear Friend: —

. . . .

I have finished my electric clock, and it operates perfectly, breaking and restoring circuit in accordance with the beats of the clock; Doctor LOCKE's objection, "deflagration," which he made the ground of his reinvention, is evidently a mere bugbear. In the meantime, Mr. AIRY, in an address to the Royal Astronomical Society, has given the whole credit of the determination of differences of longitude by means of the magnetic telegraph to *Doctor LOCKE!* What will WILKES, WALKER, LOOMIS and BACHE think of this summary and monstrous assertion?

. . . .

† The name is now spelled with a single l.

at his house, fitted up as a circuit breaker, with a tilt-hammer struck by the teeth of the escapement wheel. Mr. WALKER, also, acting for the Coast Survey, engaged the use of the line from Louisville to Pittsburg to try the experiment with Doctor LOCKE's contrivance. No astronomical nor clock signals were exchanged this evening, and no attempt was made to determine longitudes. In this experiment Doctor LOCKE's clock graduated a fillet of paper, as delivered by the MORSE register.

January 19, 1849, the first actual experiment of the automatic imprint of star signals on a time scale was made between Philadelphia and Cincinnati. The telegraph line from Philadelphia to Cincinnati was engaged for use of the Coast Survey by Mr. WALKER. The automatic clock interrupter was furnished by Doctor LOCKE, of Cincinnati. The star signals were given by Professor KENDALL at Philadelphia, and recorded at both places. The Cincinnati Observatory, in the absence of Professor MITCHELL, could not be used for the purpose of longitudes.

The longitudes of Cambridge, New York and Philadelphia were determined on the 23d of January, 1849, by star-transit signals given for the same star as it passed the meridians of these three stations. These signals were recorded at Washington, Philadelphia and Cambridge. The arrangements were under the charge of Mr. WALKER. The circuit-breaking clock was prepared by Mr. WALKER, on Doctor LOCKE's plan, and located at Philadelphia. The same clock contained a tilt-hammer interrupter for making signals by the teeth of the hour wheel every two minutes. This instrument was invented in the year 1847 by J. J. SPEED, Esq., president of the telegraph company in Detroit, Michigan.

The detection of a delay in the transmission of the galvanic inducing wave proportional to the space traversed, was made by Mr. WALKER immediately after examining and comparing together the registers of the four stations above mentioned.

The consideration of this phenomenon led him to the discovery of the velocity of the galvanic wave. A velocity of 15,400 miles per second is given by him as the most probable result.

In the summer of 1849 Professor MITCHELL proposed the use of a revolving disc of type metal to receive the records. Mr. SAXTON's plan of making the records upon a sheet of paper rolled upon a cylinder seems that which combines the most practical advantages. Mr. SAXTON proposed to break the circuit by a tilt-hammer struck by a piece of glass projecting from the middle of the pendulum which acts as a circuit breaker; he also contrived an apparatus for making on the sheet the 0^m, 5^m, 10^m, etc., by the omission of one or two breaks respectively. Mr. SAXTON's apparatus has been in use ever since at the Seaton station;* its only defect is the want of uniformity in the time of revolution of the cylinder.

On the 12th of April, 1850, Mr. BOND submitted to Professor BACHE a model of an invention made with a view to remedy this remaining defect. This instrument has been named the spring-governor.† A perfect working instrument was ordered for the use of the Coast Survey by the superintendent at that time. The model was completed and reported upon in November, 1860. [Here Mr. WALKER's printed report says, "Mr. BOND's machine surpasses in excellence all devices of the kind yet tried in the Coast Survey Service."] The cylinder, covered

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* United States Coast Survey Station in Washington.

† This instrument is now universally known as the chronograph. The only material change is one of name. The name spring-governor applies only to the regulating device.

with paper, revolves once in a minute, and measures time with the precision of an astronomical clock. The sheet when taken off, after being graduated by the clock, has the minute column vertical; the seconds are marked off horizontally on each minute scale. The eye seizes on the appropriate hour, minute and whole second, as in an ordinary astronomical table of double entry; the fraction of a second may be estimated to a tenth by the eye, or read to a hundredth by a graduated hour scale. A year's work of an ordinary observatory may be bound up in a volume of a few hundred pages, and forms a permanent and legible record of the actual dates of the imprinted transit signals. [Here Mr. Walker's printed report says, "Mr. Bond's method is likely to supersede all other methods yet known."]

By means of the line connecting the observatory of Cambridge with Boston, the time for the use of the shipping and for the railroads throughout New England is now regularly transmitted. By merely passing the circuit through the clock at Cambridge, its beats are thus given at any time (our clock has been adopted for the signals) through a distance of one or two hundred miles.

The courtesy with which the telegraph companies in different parts of the United States have met applications for the use of their lines for scientific purposes has very greatly contributed to the success which has attended these operations.

A description of the electro-magnetic apparatus which has for some time past been in use at the Harvard Observatory, has been published in the *Monthly Notices* of the Royal Astronomical Society for May, 1851.

The mechanical idea of the "spring-governor" — the regulating device of the chronograph — is to be credited to W. C. BOND and his two sons, RICHARD and GEORGE, working in concert. It is probable that the very first conception was due to RICHARD BOND. The medal of the Exhibition of 1851, and other rewards, were given to the firm of WILLIAM BOND & SON.

An extract from one of GEORGE BOND's letters on the subject may find an appropriate place here.

GEORGE BOND TO HON. WILLIAM MITCHELL.

CAMBRIDGE, December 30, 1850.

Dear Sir:—

. . . .

3d—Electro-magnetism was in use for determining differences of longitude some time before it was applied to astronomy proper. The invention was not, I take it, a sudden happy thought of any one; but has been gradually developing for two or three years. The idea first suggested had not the definiteness since realized, because everything remained to be done before it could be made practically useful.

The first step towards this end was the plan proposed by W. C. BOND for the break-circuit clock. The next desideratum was supplied by the spring-governor.

S. C. WALKER has, from the first, been actively engaged in experimenting—always in correspondence with the telegraph offices, arranging with them for the use of the lines, devising new plans for observing and agitating the subject in various ways. But I do not suppose that he knows himself whether he or W. C. B. first suggested the idea of observing by electro-magnetism. One thing I am certain of is, that we never heard the names of Professor MITCHELL and Doctor LOCKE in connection with the subject, till after the whole ground had been gone over at Cambridge.

In one of last year's numbers of the *Proceedings* of the Royal Astronomical Society, is an abstract of an address of Mr. AIRY, on observing by electro-magnetism, which stands sadly in need of a good sifting.

4th — The recording cylinder may as well be at New York as in Cambridge, provided the connection is good. Very truly yours,

G. P. BOND.

To sum up the whole matter, we may say that the "American" method of determining the difference of longitude of two places (by comparing clocks at the two stations by means of signals over a telegraphic circuit) was first employed by Admiral WILKES in 1844. The great possibilities of this method were at once seen by all astronomers. To bring the method to perfection it was necessary (*a*) to adapt the astronomical processes then in use to this particular problem. This was done by WALKER, and subsequently, under his direction, by GOULD, and by their adjuncts in the Coast Survey Service. At WALKER's death (1853) GOULD was placed in charge of the longitude bureau of the Coast Survey, and was the first to extend the method to determining differences of longitude over ocean cables (1866). The longitude of Washington from Greenwich was fixed at $5^h\ 8^m\ 12^s.39$, but the figures contain an uncorrected error, due to the application of a personal equation with a wrong sign. Subsequent longitude campaigns were made in 1870 and 1872 by officers of the Coast Survey, and the results for the longitude of Harvard College Observatory, as deduced by HILGARD, are:—

1866	H. C. O.—Greenwich	=	$4^h\ 44^m\ 30^s.99$
1870		=	30 .98
1872		=	30 .98
Which gives for the longitude of Washington			5 8 12 .09

The agreement of the three separate longitudes for H. C. O. from the three campaigns is almost too close. It is interesting to remark that in one of the later campaigns of the Coast Survey the longitude of America was determined not only from Greenwich, but from Paris as well. The difference of the two results gave, therefore, the longitude of Paris from Greenwich ($9^m\ 21^s.09$). In the British *Nautical Almanac* for 1872 this difference is given as $9^m\ 20^s.63$, so that the exact relative position of these neighboring observatories was determined through America, by Coast Survey parties, for the first time.

W. C. BOND and his son employed the method of telegraphic longitudes in the determination of the positions of Quebec, Montreal, Fredricton (N.B.), Halifax, Clinton, Portland, Bangor, etc., and Harvard College Observatory became practically the centre of longitude for the United States, though, of course, the final reference was to Washington, as it should be.

From January, 1852, onwards, time signals from a standard clock at Cambridge were *regularly* transmitted to Boston for the convenience of mariners, jewelers, etc. Such signals had been transmitted for several years previously.

The second point to be attended to was (*b*) to obtain an automatic signal from the standard clock at each station, and to transmit it to the recording apparatus. This problem was solved by SEXTON, BOND, LOCKE and others. WALKER was the first to state the requirements clearly, I believe.

The third point was (*c*) to devise a suitable registering apparatus (chronograph). Various experiments by SEXTON, LOCKE and others were of value. The chronographic disc of MITCHEL, if properly made, would have been satisfactory, though it has some inconveniences.

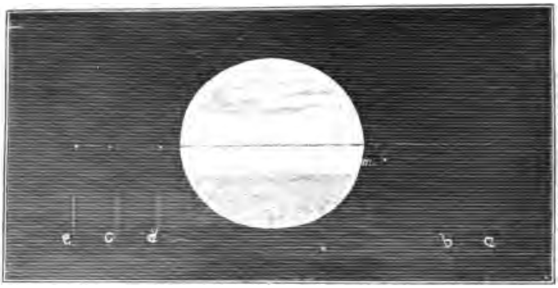
The spring-governor chronograph of the BONDS was, however, far superior in several respects to MITCHEL'S device, and with its invention the mechanical problem was completely and intelligently solved.

One of the first problems for American observers was to find out the longitude of America from Europe. When the first Atlantic cable was laid

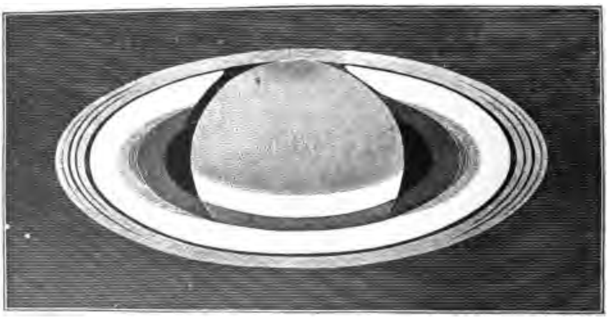
(1858) an accurate method of comparing clocks was at hand. Before that time it was necessary to use less exact methods. The telegraphic longitude of the Harvard College Observatory, referred to Greenwich, has been determined by the United States Coast Survey, from three separate campaigns, to be $4^h\ 44^m\ 30^s.98$, as we have seen. From a series of moon culminations at Harvard during the years 1843–45, it was fixed at $4^h\ 44^m\ 28^s.47$; from all the eclipses and occultations observed at Dorchester and Harvard during the years 1820–40, the longitude resulted as $4^h\ 44^m\ 32^s.16$. It is also possible to determine the longitude in a different way—namely, by actually transporting a large number of chronometers from one point to another. Such chronometric expeditions were carried out on a magnificent scale during the years 1848–55 by W. C. Bond, acting for the United States Coast Survey. The best result for the longitude from chronometers was $4^h\ 44^m\ 30^s.10$. The last three results were of the highest precision that could be attained by the old methods, particularly the chronometric value, based as it is on many voyages and scores of chronometers. The difference between the early values and what is now known to be the final longitude is to be attributed entirely to (unavoidable) defects in methods. The observations before 1866 were very numerous and very carefully made (the greater part of them by W. C. BOND), and they were elaborately reduced, partly by GEORGE BOND, and finally discussed by WALKER. The following paragraphs from LOOMIS's *Recent Progress of Astronomy in the United States*, p. 301 (1856), describe the chronometric campaigns in detail.

Advantage has been taken of the frequent passage of steamers between Boston and Liverpool to make a thorough comparison of the times of those ports by means of chronometers. For this purpose, as soon as a steamer arrives in Boston, its chronometers are taken to Cambridge Observatory for comparison, where they remain until the steamer is ready to return.* Upon arriving in Liverpool, the chronometers are taken to the Liverpool Observatory and their errors determined. This method of comparison has been systematically pursued since 1844. During the year 1846, forty-two such comparisons were made. In 1848 the longitude of the Cambridge Observatory from Greenwich was determined by Mr. BOND, from the transportation of 116 chronometers, in thirty-four voyages of the Cunard steamers from Liverpool to Boston, to be $4^h\ 44^m\ 30^s.5$. The longitude deduced from lunar occultations and solar eclipses is $4^h\ 44^m\ 31^s.9$. During the year 1849 eighty-seven additional comparisons were made, the results of which differ nearly two seconds of time from those previously obtained by astronomical observations. The mean result of 175 chronometers was $4^h\ 44^m\ 30^s.1$, and it was believed that this result could not be one second in error. The final result of the chronometric expeditions of

* The chronometers were, in fact, taken to the offices of WM. BOND & Son in Boston, and remained there till the next voyage. Telegraphic signals from the Cambridge Observatory clock were received at the Boston office.



Saturn: August 30, 1848.



Saturn: January 9, 1855.

1849, 1850 and 1851 was 4^h 44^m $30^s.66$. During the progress of these expeditions, more than four hundred exchanges of chronometers have been made.

The large equatorial was employed during the years 1847–56 in a study of the planet *Saturn*, which led to remarkable results. In 1848 the ring was presented to the earth and sun so as to disappear, or to be visible simply as a thin bright line; while in 1856 it was much inclined and fully illuminated, so as to assume its maximum width. The year 1848 was a suitable time to search for new satellites therefore, and on September 15th and 16th GEORGE BOND mapped the known satellites, together with a faint object, which was, in fact, a satellite, and which, on the other hand, might have been a small star. On September 18th WILLIAM BOND observed early in the evening, but the star was not noticed. Later on his son made a new diagram, and the faint object was again recorded. On September 19th diagrams were made by both observers, which contained the faint object in question, and measures conclusively proved it to be a new satellite, since it moved among the stars along with the planet. This was the new satellite *Hyperion*.*

*** W. C. BOND is often named (alone) as the discoverer. The honor really belongs to both observers. If the discovery is to be assigned to a single person, then it must be credited to GEORGE BOND.**

The satellites of *Saturn*, arranged in the order of their distances (semi-diameter of the ball of *Saturn* = 1.0), are—

1. *Mimas*,	distance 3.3;	discovered	by	HERSCHEL	in 1797
2. *Enceladus*,	" 4.3;	"	"	"	in 1789
3. *Tethys*,	" 5.3;	"	"	CASSINI	in 1684
4. *Dione*,	" 6.8;	"	"	"	in 1684
5. *Rhea*,	" 9.5;	"	"	"	in 1672
6. *Titan*,	" 20.7;	"	"	HUYGHENS	in 1755
7. *Hyperion*,	" 26.8;	"	"	the BONDS	in 1848
8. *Iapetus*,	" 64.4;	"	"	CASSINI	in 1671

The names of the BONDS will be forever associated with those of their great predecessors, and with the planet *Saturn* on more than one account.*

The following letter from WILLIAM BOND to the President of Harvard University gives an account of the discovery of *Hyperion*.

OBSERVATORY, CAMBRIDGE,
September 25, 1848.

Dear Sir:—

On the evening of the 16th of this month a small star was noticed, situated nearly in the plane of *Saturn's* ring, and between the satellites *Titan* and *Iapetus*. It was regarded at the time as accidental. It was, however, recorded, with an estimated position in regard to *Saturn*.

The next night favorable for observation was the 18th, and while comparing the relative brightness of the satellites, the same object, similarly

*Mr. WILLIAM LASSELL, the discoverer of the satellite of *Neptune* and of two faint satellites to *Uranus*, is also an independent discoverer of *Hyperion*. He first noticed it on September 18th, and on the following night satisfied himself that it was attendant on the planet.

situated in regard to the planet, was again noticed, and its position more carefully laid down. But still, at the time, we scarcely suspected its real nature.

From accurate measurement on the evening of the 19th, the star being found to partake of the retrograde motion of *Saturn*, that portion of the heavens toward which the planet was approaching was carefully examined, and every star near its path for the two following nights laid down on a diagram, and micrometric measures of position and distance with objects in the neighborhood were taken.

The evening of the 20th was cloudy. On the 21st the new satellite was found to have approached the primary, and it moved sensibly among the stars while under observation. Similar observations were repeated on the nights of the 22d and 23d. Its orbit is exterior to that of *Titan*. It is less bright than either of the two inner satellites discovered by Sir WM. HERSCHEL. Respectfully,

W. C. BOND.

TO PRESIDENT EVERETT.

The ring of *Saturn* was observed by GALILEO, who could not explain the phenomenon which he saw. It was left to HUYGHENS to describe it accurately: "*Saturn* is girdled by a thin, plane ring, nowhere touching, inclined to the ecliptic." CASSINI had shown that the ring was divided into two parts by a broad, empty division. In 1850 (October 10) GEORGE BOND recorded the presence of a third faint, veil-like ring attached to the inner one of the known rings, but much fainter, which has been called the *dusky-ring* or *crape-ring* of *Saturn*.* It is easily seen in comparatively small telescopes, and had been seen previously by other observers, though no proper attention was paid to the observation. Subsequent observations at Cambridge and elsewhere fully confirmed its existence, and measures fixed its dimensions.

The oppositions of 1847–56 were devoted by the BONDS to observations, drawings and measures of *Saturn*, and a vast mass of material accumulated, the most important part of which is printed in the Harvard College Observatory *Annals*, Volume II. No such thorough-going examination of a planet had ever been made. The observations just named, the discovery of the dusky-ring, the observation of various vacant (dark) spaces in the bright rings themselves, parallel and similar to the principal division, the fact that the dusky-ring was transparent (as the edges of the ball of *Saturn* could be seen through it), and other points, called attention to the necessity for a complete theory of the constitution of the rings to account for the extraordinary character of the phenomena.

In the *Astronomical Journal* of May 2, 1851, GEORGE BOND has a paper, which was first read at

**Annals* H. C. O., Vol. II, p. 46. The first drawing of the new ring was made on November 11th by GEORGE BOND. It was first seen elsewhere by Rev. W. R. DAWES, November 25th.

the meeting of the American Academy on April 15th, in which he recites the previous theories of the rings, and, using the analysis of LAPLACE as a starting point, shows that the ring cannot be a continuous solid, and that the hypothesis, that the whole ring is in a fluid state, "presents fewer difficulties."

In the *Journal* of June 16th Professor PEIRCE prints a paper on the constitution of *Saturn's* ring, which was read at the meeting of the American Association for the Advancement of Science, in Cincinnati, and which, in substance, was given as a supplement to the paper of BOND on April 15th, in the form of verbal remarks at the meeting of the American Academy in Boston. In his work on *Analytical Mechanics* PEIRCE developed his theory more fully.

Professor PEIRCE says: "Mr. BOND'S argument for the fluidity of *Saturn's* ring is chiefly derived from observation; whereas I have undertaken to demonstrate from purely mechanical considerations, that it cannot be solid." "Mr. BOND'S bold and ingenious theory" it is called.

The following brief historical statement from NEWCOMB'S *Popular Astronomy** will show that, while both authors made a distinct step forward in

* The foot-notes are not Professor NEWCOMB'S.

proving that *Saturn's* rings could not be a continuous solid, both came to an erroneous conclusion in supposing them to be fluid.

Newcomb says: "The astronomers of two centuries ago saw nothing surprising in the fact of a pair of rings surrounding a planet and accompanying it in its orbit, because they were not acquainted with the effects of gravitation on such bodies as the rings seemed to be. But when Laplace investigated the subject he found that a homogeneous and uniform ring surrounding a planet could not be in a state of stable equilibrium.* Let it be balanced ever so nicely, the slightest external force, the attraction of a satellite or of a distant planet, would destroy the equilibrium, and the ring would soon be precipitated upon the planet. He therefore remarked that the rings must have irregularities in their form, such as Herschel supposed that he had seen; but he did not investigate the question whether, with those irregularities, the equilibrium would really be stable. The question was next taken up in this country by Professors Peirce and Bond. The latter started from the supposed result of observations—that new divisions show

* *Mécanique Céleste*, Bowditch's translation, Vol. II, pp. 492–519, where Laplace's conclusion is, that "the rings are irregular solids of unequal widths in the different parts of their circumferences, so that their centers of gravity do not coincide with their centers of figure."

themselves from time to time in the ring, and then close up again.*

He thence inferred that the rings must be fluid, and, to confirm this view, he showed the impossibility of even an irregular solid pair of rings fulfilling all the necessary conditions of stability and freedom of motion.

Professor PEIRCE, taking up the same subject from a mathematical point of view, found that no conceivable form of irregular solid ring would be in a state of stable equilibrium; he therefore adopted BOND'S view that the rings were fluid. Following up the investigation, he found that even a fluid ring would not be entirely stable without some external support, and he attributed that support to the attractions of the satellites; but as LAPLACE did not demonstrate that irregularities would make the ring stable, so PEIRCE merely fell back on the attractions of the satellites as a sort of forlorn hope, but did not demonstrate that the fluid ring would really be stable under the influence of their attraction. Indeed, it now seems very doubtful whether this attraction would have the effect supposed by PEIRCE.

The next, and we may say the last, important

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* This is undoubtedly the case, as is shown by BOND'S and other observations. See the drawing of *Saturn* of 1855 in this book, page 251.

step was taken by Prof. J. CLERK-MAXWELL, of England, in the ADAMS' prize essay for 1856.* He brought forward objections which seem unanswerable against both the solid and the fluid ring, and revived a theory propounded by CASSINI about the beginning of the last century. This astronomer considered the ring to be formed by a cloud of satellites, too small to be separately seen in the telescope, and too close together to admit of the intervals between them being visible. This is the view of the constitution of the rings of *Saturn* now most generally adopted. The reason why the ring looks solid and continuous is, that the satellites are too small and too numerous to be seen singly. They are like the separate little drops of water of which clouds and fog are composed, which to our eyes seem like solid masses. In the dusky ring the particles may be so scattered that we can see through the cloud." The views of MAXWELL and HIRN, which were derived from pure theory, have recently received a striking confirmation from the spectroscopic observations of Doctor KEELER at Allegheny, and of Professor CAMPBELL at the Lick Observatory.

Having this general view of the whole history of the researches on the constitution of *Saturn's* rings from HUYGHENS' time until now, we can see

* And, independently, by G. A. HIRN. MAXWELL's essay is reprinted in his *Scientific Papers*, Vol. I, p. 288.

what BOND's share in the progress has been. LAPLACE and HERSCHEL* had left it possible to believe that the rings were solid. BOND's observations (1848–51) made it evident that this could not be the case. The memoirs of PEIRCE and BOND suggested the subject of the ADAMS' prize essay of 1856.

Astronomers will be interested in noting that the method of determining the parallax (of *Mars*) by differential observations in right ascension east and west of the meridian, was independently proposed by the BONDS, and carried into effect at the Harvard College Observatory in 1849–50, ten years before its adoption elsewhere. Chapter I contains a letter of W. C. BOND on this point.†

During the years 1855 to 1872 the observatory published in its *Annals* various catalogues of faint stars situated near to the equator. The first catalogue was of 5500 stars, the second of 4484 stars, the third of 6100 stars, or 16,084 stars in all. The places of these fixed stars were determined, so that any moving star (planet) should be detected and discovered. The first object was to discover new

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* "We may certainly affirm that the ring is no less solid than the ball." — *Philosophical Transactions* of the Royal Society of London, 1790, p. 5.

† The method was first employed by CASSINI and FLAMSTEED in 1672. BOND's results are given in the *Astronomical Journal*, Vol. 5, p. 53.

asteroids, though such zones have other uses. Two asteroids were discovered at Harvard College Observatory in 1861 and 1862. The first thirty asteroids were discovered in Europe. No. 31 (*Euphrosyne*) was found, in 1854, by FERGUSON, at Washington; No. 50 (*Virginia*) by FERGUSON, in 1857; No. 55 (*Pandora*) in 1858, by SEARLE, at the Dudley Observatory; No. 60 (*Echo*) by FERGUSON, in 1860; No. 66 (*Maia*) by H. P. TUTTLE, at the Harvard College Observatory, in 1861; No. 72 (*Feronia*), 1861, was the first of the forty-eight asteroids discovered by PETERS, at Clinton; No. 73 (*Clytie*) was found by TUTTLE, at Harvard, in 1862; No. 79 (*Eurynome*) was the first of the twenty-two asteroids found by WATSON, at Ann Arbor; and from 1862 onwards PETERS and WATSON, in America, PALISA, LUTHER, BORRELLY and others, abroad, swelled the list until it has attained its present large proportions.

In the earlier years of the observatory GEORGE BOND searched assiduously for comets; and eleven comets were independently discovered by him. He was anticipated, however, in his discovery of unexpected comets, by European observers, by a few days, or even hours, in every case except that of the second comet of 1850, discovered August 29th. The return of ENCKE'S comet, in 1848, was observed by

BOND two or three days before it was seen in Europe. On June 2, 1845, and April 11, 1849, comets were discovered by him on the nights of their discovery in Europe, but a few hours later. In later years BOND ceased to search for new comets, as he found that his eyes were suffering from the strain.

He was assiduous, also, in calculating the elements of the orbits of new comets, and had computed more than a score of comet orbits before 1851. The first orbit of *Hyperion* was due to him, also. Two of the TUTTLES (C. W. and H. P.) were successively assistants at the observatory, and five comets were discovered by the latter. I believe it is not generally known that HORACE TUTTLE discovered the great comet of 1858 sometime before it was found by DONATI. It was very faint, and among a number of nebulæ. Before the discovery was made absolutely sure by measures of position, it was announced from Europe.

The invention of photography by DAGUERRE (in 1839) opened the way to an immense step in astronomical progress. The daguerreotype was first employed to photograph the moon by Prof. JOHN WILLIAM DRAPER, of New York, on March 23, 1840, but the exposure was very long (twenty minutes). A daguerreotype of the solar eclipse of 1842 was taken by MAJOCCHI in Milan, and of that of 1851 by BUSCH of Kœnigsberg. In April, 1845, on the advice of ARAGO, MM. FOUCAULT and FIZEAU, of the Paris Observatory, made daguerreotypes of the sun. In 1849 (December 18) the first daguerreotype of the moon was made at the Harvard College Observatory by Messrs. WHIPPLE and BLACK, under the direction of the BONDS. The telescope was not corrected for the photographic rays, and, therefore, under no circumstances could perfect results be obtained, but after a few trials pictures of the moon were secured, which were exceedingly interesting. The best of them were taken to Europe by GEORGE BOND in 1851, and created a veritable *furore*.* The first photograph of a star (α *Lyræ*) was made at the Harvard College Observatory on July 17, 1850, and enough was done to show that the photographic method was very suitable for fixing the relative positions of double stars, for the determination of stellar parallax, for making star maps, etc. A letter of GEORGE BOND'S (dated July 6, 1857), on the general subject of astronomical photography, is printed in full in Chapter IV. A part of it is reprinted here, and it will be seen that in 1857 BOND thoroughly understood the whole question. He is the father of celestial

* In 1852 the exposure on the full moon was between five and six seconds for the best results.

photography. Doctor LEWIS RUTHERFORD's splendid work did not begin till later. His observatory was built in 1856; he turned his attention to photography in 1858; and his earliest photograph of the moon was taken in June of the latter year. Doctor HENRY DRAPER was still later in the field, 1863. Doctor DE LA RUE's first photographs of the moon were made in 1852, after having seen the Harvard College results. It was not until 1857 that he began his series of most successful lunar photographs. The suggestion to use the large telescope for photography was made by the younger BOND, and most of the work at the telescope was done by him. The photographic manipulations were all made by Messrs. WHIPPLE and BLACK, who gave their time and service without payment in a very disinterested manner.

After reciting the history of the subject, BOND's letter goes on to say:—

The results already obtained are of the highest interest, and suggest possibilities in the future which one can scarcely trust himself to speculate upon. Could another step in advance be taken equal to that gained since 1850, the consequences could not fail of being of incalculable importance in astronomy. The same object, α *Lyræ*, which in 1850 required 100 seconds to impart its image to the plate, and even then imperfectly, is now photographed instantaneously with a symmetrical disc, perfectly fit for exact micrometer measures. We then were confined to a dozen or two of the brightest stars, whereas now we take all that are visible to the naked eye. Even from week to week we can distinguish decided progress. Of the beauty and convenience of the method, you will scarcely form a correct idea without witnessing for yourself, which I hope you will be able to do before long. On a fine night the amount of work which can be accomplished, with entire exemption from the trouble, vexation and fatigue that seldom fail to attend upon ordinary observations, is astonishing. The plates, once secured, can be laid by for future study by daylight and at leisure. The record is there, with no room for doubt or mistake as to its fidelity. As yet, however, we obtain images only from stars to the sixth magnitude, inclusive. To be of essential service to astronomy, it is indispensable that great improvements be yet made, and these I feel sure will not be accomplished without a deal of experimenting.

But could we but press the matter on, we should soon be able to say what we can and what we cannot accomplish in stellar photography. The latter limits we certainly have not yet reached. At present the chief object of attention must be to improve the sensitiveness of the plates, to which, I am assured by high authorities in chemistry, there is scarcely any limit to be put in point of theory. Suppose we are able finally to obtain pictures of seventh-magnitude stars. It is reasonable to suppose that, on *some lofty mountain and in a purer atmosphere*, we might, with the same telescope, include the eighth-magnitude. To increase the size of the telescope three-fold in aperture is a practicable thing, if the money can be found. This would increase the brightness of the stellar images, say eight-fold, and we should be able then to photograph all the stars to the tenth and eleventh magnitude, inclusive. There is nothing, then, so extravagant in predicting a future application of photography to stellar astronomy on a most magnificent scale. It is, even at this moment, simply a question of finding one or two hundred thousand dollars to make the telescope with and to keep up the experiments.

What more admirable method can be imagined for the study of the orbits of the fixed stars and for resolving the problem of their annual parallax than this would be, if we could obtain the impressions of the telescopic stars to the tenth magnitude? Consider, too, that groups of ten, or fifty even, if so many occur in the compass of the field, will be taken as quickly as one alone would be—perhaps in a few seconds only—and each mapped out with unimpeachable accuracy.

I have not alluded to two important features in stellar photography. One is, that the intensity and size of the images, taken in connection with the length of time during which the plate has been exposed, measure the relative magnitudes of the stars. The other point is, that the measurements of distances and angles of position of the double stars from the plates, we have ascertained by many trials on our earliest impressions, to be as exact as the best micrometric work. Our subsequent pictures are much more perfect, and should do better still.* . . .

HARVARD COLLEGE OBSERVATORY, 1857, July 6.

Every line of this letter shows that BOND understood the entire capabilities of the photographic

* In the Observatory Notebook of April 30, 1857, BOND says: "Any considerable improvement in the sensitiveness of the present photographic processes will revolutionize practical astronomy — such an improvement as will enable us to take stars of the tenth magnitude in a second or two." It will be observed that mountain observatories are proposed; and that the last paragraph contains the germ of a photographic photometry. BOND was the first observer to determine the relative brightness of stars by photography. See *Astronomische Nachrichten*, vol. 49, col. 81.

methods, in 1857, exactly as they are understood to-day. His knowledge was based on hundreds of experiments, extending over eight years. This letter was first printed in the *Publications* of the Astronomical Society of the Pacific, in November, 1890. Since that time, certainly, there has been no excuse for denying to Bond his just deserts. It is to him that astronomers must look as the true founder of photographic astronomy. The path that he traced out has been followed with brilliant success by a host of investigators of talent or of genius, and nowhere with more success than at the Harvard College Observatory in recent years. But it is the simplest justice to acknowledge his priority. This can be done without in the least detracting from a profound admiration for those who followed in his footsteps.

The third volume of the *Annals* of the Harvard College Observatory, published in 1862, is entirely devoted to the observations made by Bond (and others) upon the great comet of 1858, and to a discussion of the comet with the object of throwing light upon the mysteries of its physical constitution. The text is accompanied by a series of very remarkable steel plates (one of which is reproduced in the frontispiece of the present volume), which represent in the most faithful manner the appearances of the comet to the eye and in the telescope. Nothing of anything like this excellence had ever been done before; now that we have photography to aid us, nothing of the sort will ever be done again. It stands alone, and is and will remain unique of its class.

The great comet of 1858 was discovered while it was yet a faint object, by Doctor DONATI, of Florence. Its whole history was remarkable. It was visible to the naked eye for no less than 112 days (August 19th to December 9th), and in the telescope 275 days (June 2, 1858, to the 4th of the following March). Its motions were such as to present it, a splendid object in the west, near the bright star *Arcturus* during its period of greatest brilliancy. It was so bright that processes taking place within its head and the surrounding envelopes were readily followed. Such an opportunity for a thorough investigation had not presented itself for long years, and there has been no comet since 1858 which was anything like so favorably situated. The opportunity was unique. The comet was studied by astronomers all over the world, and their results were presented in the fullest form. There is no memoir approaching BOND's in completeness. The fundamental mystery of cometic constitution was not solved by him, nor is it solved to-day; but every phenomenon of the comet's development was elaborately described, and many important and original conclusions were drawn. Of the fifteen chapters of the volume, ten were devoted to the phenomena of the tail (tails) and three to the nucleus and envelopes. The form of the head of the comet was elaborately studied, and its shape determined to be that of a catenary curve, rather than that of a parabola. The phenomena of the production of the successive envelopes from the nucleus led BOND to conclusions which were doubted at first, but which were subsequently verified (by him) in the case of the bright comet of 1861.* The next bright comet that appears (may it come soon) will be studied with the enormous advantage of photographic registration. We have already reason to believe that such a study will go far towards providing a satisfactory theory of the constitution of these enigmatic bodies.

The memoir of BOND will provide a ready test of any such complete theory, which must not only account for the observed appearances, but also for those of the comet of 1858, as laid down in this volume. It is not practicable to convey to the non-professional reader an adequate idea of the merit

* BOND was in the habit of saying that this comet so fully upheld and illustrated his theory of the formation of the envelopes, etc., that, once having the idea, he could afford to throw away all his work on the DONATI Comet, and deduce every conclusion from the phenomena presented by the comet of 1861.

of this great work without going into details for which there is no room here. It may suffice to say, that the opinion of astronomers is unanimous that no such satisfying investigation of a comet is known, and that the highest award of the Royal Astronomical Society of London (its gold medal) was rightly adjudged to BOND for his memoir.

The engraving which is here reproduced may serve as an index of the beauty as well as of the completeness of this monumental work, when it is remembered that this plate is but one among many.

The list of GEORGE BOND's papers, in the Appendix, contains several on purely mathematical subjects, which can only be referred to here. His paper on Mechanical Quadratures was printed in 1849, when he was twenty-four years of age, and it anticipates, by two years, a part of the papers of ENCKE on the same subject. A letter of ENCKE's (dated May 19, 1852) will be found in Chapter IV, in which BOND's priority is very handsomely acknowledged.

In September, 1860, BOND communicated to the American Academy two papers on photometry, which demand a notice here. Their titles were, (1) On the Light of the Moon and of the Planet *Jupiter*, (2) Comparison of the Light of the Sun and Moon. The correspondence in Chapter IV contains some references to these works. The extracts which follow will exhibit the plan of the research.

On the 22d of March, 1851, several daguerreotype pictures of *Jupiter* were obtained on plates exposed at the focus of the great refractor. The belts were faintly indicated; but the most interesting fact in connection with the experiment, apart from its having been, as is believed, the first instance of a photographic impression obtained from a planet, was the shortness of the time of the exposure, which was nearly the same as for the moon, whereas, considering the relative distance of the two bodies from the sun, it was to have been expected that the light of the moon would have had twenty-seven times more intensity than that of *Jupiter*, supposing equal capacities for reflection. The experiments were repeated on the 8th and 9th of October, 1857, by Mr. WHIPPLE, using the collodion process, with a like result.*

From many experiments, BOND found that "*Jupiter* reflects, out of a given quantity of incident light, *fourteen* times more of the chemical [photographic] rays than the moon does. The distribution of light over the discs of *Jupiter* and the moon presented, in the photographs, a decided contrast; the former is brightest near the center, the latter near the margin."†

BOND's experiments show that the moon absorbs about ten parts out of every eleven of the

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* The energetic action of *Jupiter's* light was also remarked by Dr. DE LA RUE, in England, in December, 1857.

† *Jupiter* has a dense atmosphere; the moon has none, or very little. Photographs of *Mars* are like those of the moon, not like those of *Jupiter*, in this respect, as was remarked by BOND, and this fact of observation has an important bearing on the question of the atmosphere of *Mars*. It indicates that the atmosphere of that planet is exceedingly thin.

light which falls upon it, while *Jupiter* actually reflects, according to his figures, more light than it receives—i. e. is to a small degree self-luminous. Measures were made of the relative brightness of *Venus* and *Jupiter*, etc., using the flame of a lamp as a standard of comparison.

In his second paper, the brightness ef the sun and moon were compared through the intermediation of a Bengal light flame. His conclusion was, that the light of the sun (seen visually, not photographically) was equal to that of 470,980 full moons. In such delicate experiments as these, where the physiology of the eye plays its part, as well as the physics of the apparatus employed, there are many pitfalls, all of which have not been avoided in the research in question. The flames of the standard lamp and of the Bengal light are by no means constant, even for short times. The background on which the sun is projected is very different from the night-sky behind the moon, and, finally, the *colors* of sun, moon, *Jupiter* and *Mars* are very different. Some of these points are discussed by Bond, and he gives his final conclusions with due reserve, also. The same subject has been carefully worked over by Zoellner with exceeding care. In the nature of the case, all results of this sort are subject to large errors (say five *per cent.*, or even ten). Zoellner's figure for the relative brightness (visual) of the sun and moon is 618,000; and he finds that while the moon reflects $\frac{17}{100}$ of the light which falls upon it * (such light as the eye can appreciate), *Jupiter* reflects $\frac{64}{100}$, or three and one half times as much. BOND's conclusion was, that *Jupiter* reflects *eleven* times as much (visual), and *fourteen* times as much (photographic).

While BOND's general conclusions are correct, it is certain that his figures are too high. These memoirs are of considerable value in several directions, especially for BOND's conclusions as to the nature of the lunar surface.

"If the full moon were polished perfectly smooth, we should not see its limb [circumference] at all, but only an image of the sun, formed by reflection from its surface; the visibility of its outline, then, is entirely due to its asperities, and the particular way in which these are disposed will have a great effect on the distribution of its light." "It may be worth while to notice, too, that the moon is little, if at all, exposed to the disintegrating action of water or of an atmosphere; hence it is not unlikely that its surface, instead of presenting the dull, weather-worn aspect of the earth, may rather resemble that of bright, fresh-fractured rocks, with their usual crystalline lustre."

These two papers may be called highly suggestive, rather than final.

* About the same amount as is reflected by dark sandstone rock.

In the year 1848 William Bond had printed a paper which contained a part, only, of his observations on the great nebula of *Orion*. The paper had merit, but it was open to criticism in several respects. Otto Struve, in his memoir on the same object, criticised it with some severity, and it was partly on this account that George Bond took up the study of the nebula of *Orion* in 1857. It was his object to make a thorough-going investigation of the nebula in all respects, to vindicate the reputation of the observatory and to leave behind him a complete monograph. The observations on the nebula were interrupted by the work on the Comet of Donati, and were not resumed for some years.

The work was never finished, though it was pursued by Bond with feverish anxiety in the midst of hindrances of various kinds, and when his constitution was already broken by a mortal disease. After his death his observations were edited by Professor Safford, some time assistant in the observatory, and they were printed in Volume V of the *Annals*. Even in this fragmentary and unsatisfactory form, Bond's memoir on the nebula is far the most complete that exists. If he had lived to finish it, we should have had another model to follow, like the monograph on the comet of 1858.

During the years 1874–80 I employed the 26-inch telescope at Washington, when it could be spared to me, in an examination of the nebula of *Orion*,* and thus became entirely familiar with the work of all the observers who have examined this brilliant object, from the time of HUYGHENS (1656) down to the present day. Their published observations were studied in detail, and in the case of BOND I had the advantage, not only of his printed observations, but also of his manuscript notebooks, which were kindly put at my disposition by Professor PICKERING, director of the Harvard College Observatory. In many places in my *Monograph*, I have expressed my admiration for the thoroughness of BOND's work, which was done with a 15-inch telescope and revised by me with the Washington instrument, which collected three times as much light. The following quotation from my paper of 1880 will serve to show the judgment I was led to form of BOND's exactness and minute care and accuracy in his admirable study of the nebula.

"I am acquainted with but one drawing of the nebula which is entirely above criticism—that of the late G. P. BOND. He was himself a skilled artist, and he had been familiar with the nebula for fifteen or twenty years. He made scores of drawings, in white on black, and the reverse, in colors, etc. Each of these was revised and re-revised many times. The final drawing in water-color

* Monograph of the central parts of the nebula of *Orion*, *Washington Astronomical Observations* for 1878, Appendix I.

was copied by Mr. Watts, a skillful engraver, who himself was extremely familiar with the nebula, from repeated views and studies of it through the Harvard refractor. The revisions of the original plate lasted many months, and I have myself examined from fifteen to twenty '*final*' revises of the plate. Color, form and relative brilliancy were all successively and exhaustively criticized, and Professor Bond expressed himself as fully satisfied with the plate in every essential feature. Add to this, that with the exception of a few points, elsewhere considered, this engraving has been constantly satisfactory to me in my very frequent comparisons of it with the nebula, even under the severest criticism which I could apply."

Bond's catalogue of 1101 stars in the nebula was carefully revised for the portions which I had under examination, and his conclusions as to situation, brightness, etc., of the stars, were verified. His catalogue, made with a 15-inch telescope, contains almost every star visible in the much more powerful instruments used by Lassell, Lord Rosse and myself. The first photograph of the nebula of *Orion* was made by Dr. Henry Draper in 1880, and in 1882 he obtained (with an exposure of 137 minutes) a most satisfactory representation of its central regions. Such an autographic chart affords a severe test of all previous visual work, such as that of Bond. A careful comparison of the photographs with Bond's drawing only shows in a new light the astonishing fidelity of the latter.

APPENDIX

LIST OF THE SCIENTIFIC WRITINGS OF WILLIAM CRANCH BOND

(Compiled by Mrs. Richard F. Bond)

Occultations of *Aldebaran* and *σ Leonis*, observed at Dorchester, 1829–30.—*Astr. Nachr.* viii, 1831, pp. 351–354; mentioned, also, in a letter from R. T. Paine, *A. N.* viii, pp. 349, 350; noticed, also, by Wurm in *A. N.* ix, pp. 138–140.

Occultations and Eclipses, observed at Dorchester, Mass.—*Am. Acad. Mem.*, New Ser. i, 1833, pp. 79–83.

Observations on the Comparative Rates of Marine Chronometers.—*Am. Acad. Mem.*, New Ser. i, 1833, pp. 84–90.

A Chart, representing the extraordinary variations of the magnetic declination during the term day, on the 29th of May last [1840], prepared from observations at the magnetic observatory at Cambridge. —*Am. Phil. Soc. Proc.* i, 1841, p. 293; presented by Professor Bache at the meeting of Nov. 6, 1840.

Transit of *Mercury*, May 8, 1845.—*Am. Acad. Proc.* i, 1848, pp. 14–16; read by Professor Peirce, Aug. 12, 1846.

Moon Culminations, observed at Cambridge Observatory, corrected for collimation, level and azimuthal deviation of the transit instrument, and for clock rate and error on sidereal time.—*Am. Acad. Proc.* i, 1848, pp. 5–13; read by Professor Peirce, Aug. 12, 1846.

Transit of *Mercury*, May 8, 1845, pp. 14–16; Observations on the Comets of 1845 and 1846, pp. 17, 18; Solar Eclipse of May, 1845, p. 19; Solar Eclipse of Apr., 1846, pp. 20, 21; Notes on Meteors, pp. 21, 22.—*Am. Acad. Proc.* i, 1848, pp. 14–22; read by Professor PEIRCE, Aug. 12, 1846.

Observations on the New Comet [discovered by DE VICO], first seen in this country by my son, GEORGE P. BOND, Feb. 26, 1846.—*Astr. Nachr.* xxiv, 1846, pp. 91, 92.

Observations on the New Planet [*Neptune*].—*Astr. Nachr.* xxv, 1847, pp. 231–234, 301, 302.

Observations and Elements of DE VICO's Fourth Comet (Feb. 20, 1846).—*R. A. S. Monthly Not.* vii, 1847, pp. 92, 93, 187. (First seen in America by G. P. B., Feb. 26, 1846.)

Observations of *Neptune*.—*R. A. S. Monthly Not.* vii, 1847, pp. 157–225, 256–307.

Observations of Comet of May 19, 1846.—*R. A. S. Monthly Not.* vii, 1847, pp. 187–188.

Observations of HIND's Second Comet, March, 1847.—*R. A. S. Monthly Not.* vii, 1847, p. 273.

Observations on the Planet *Neptune*, 1846–47.—*Am. Acad. Proc.* i, 1848, pp. 50, 51; communicated Jan. 27, 1847.

Observations on the Comet of March 4, 1847, pp. 69, 70; Moon Culminations, observed at Cambridge (1840–42), pp. 70–103; Moon Culminations, observed at Dorchester (1838–39), pp. 104–128.—*Am. Acad. Proc.* i, 1848, pp. 69–128; communicated Mar. 16, 1847.

Observations on the Planet *Neptune*, near its quadrature, p. 168; Observations on MAUVAIS's Comet of July 4, 1847, pp. 169, 170; Moon Culminations, observed at Cambridge (1846–47), pp. 170–174, 175–177.—*Am. Acad. Proc.* i, 1848, pp. 168–177; communicated Oct. 5, 1847.

Observations on Miss MITCHELL's Comet of Oct. 1, 1847, p. 183; Observations on LASSELL's Satellite of *Neptune*, p. 184.—*Am. Acad. Proc.* i, 1848, pp. 183–184; communicated Nov. 2, 1847.

Nebula in *Orion*, resolved by the new telescope in the observatory of Harvard College.—*Am. Journ. Sci. and Arts*, Ser. 2, iv, 1847, pp. 426, 427.

Description of the Nebula about the Star *θ Orionis*.—*Am. Acad. Mem.*, New Ser. iii, 1848, pp. 87–96, *plate;* read before the Academy, Apr. 3, 1848.

Results of Recent Observations on the Planet *Jupiter* and on the Nebulæ *Herschel*, Nos. 1357 and 1376, and the Great Nebula of *Orion*.—*Am. Acad. Proc.* i, 1848, pp. 325, 326; communicated Mar. 7, 1848.

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Observations of the Comet of Oct., 1847 (MARIA MITCHELL'S), pp. 287, 288; Observations on LASSELL'S Satellite of *Neptune*, Beilage zu No. 618, Oct., 1847.—*Astr. Nachr.* xxvi, 1848, pp. 287, 288, and Beilage.

Notice of the Discovery of a Comet by G. P. BOND [the fifth discovered by him], July 14, 1847.—*Astr. Nachr.* xxvi, 1848, pp. 171–172 [MAUVAIS'S Comet].

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Description of the Observatory at Cambridge, Mass.—*Am. Acad. Mem.*, New Ser., iv, 1849, pp. 177–188, *plates;* communicated to the Acad. Nov. 8, 1848.

Discovery of an Eighth Satellite of *Saturn*, Sept. 16, 1848.—*Astr. Nachr.* xxviii, 1849, pp. 25, 26.

Letter to the Secretary, concerning the proposed chronometric expedition to determine the longitude of the Observatory of Harvard College.—*R. A. S. Monthly Not.* ix, 1849, p. 151.

———Letters on Same Subject to Mr. HARTNUP, of the Liverpool Observatory.—*R. A. S. Monthly Not.* xv, 1855, p. 215.

MAUVAIS's Third Comet, Apr. 21, 1848.—*R. A. S. Monthly Not.* ix, 1849, p. 10.

Observations on HIND's Changing Star, and loss of three stars near *Procyon*, in 1848.—Mentioned, but not given in full, in *R. A. S. Monthly Not.* ix, 1849, p. 18. In xi, pp. 20–27, G. P. B's discovery of the ring of *Saturn* is discussed, in connection with observations of LASSELL and DAWES; and in xii, p. 155 (1852), a note from W. C. B. is quoted in regard to the divisions of the ring.

ENCKE's Comet, Aug.–Nov. 1848.—*R. A. S. Monthly Not.* ix, 1849, pp. 10, 106–107.

PETERSEN's Second Comet [discovered independently by G. P. B., Nov. 25, 1848].—*R. A. S. Monthly Not.* ix, 1849, pp. 26, 107.

SCHWEIZER's Comet [discovered a few hours later by G. P. B., April 11, 1849].—*R. A. S. Monthly Not.* ix, 1849, pp. 128, 162, 163.

Observations on FAYE's Comet.—*R. A. S. Monthly Not.* xi, 1850–51, p. 63.

Appearance of *Saturn's* Ring, etc.,—*R. A. S. Monthly Not.* x, 1849–50, pp. 16–22.

Observations of *Parthenope.*—*R. A. S. Monthly Not.* xi, 1850–51, p. 9.

Observations of PETERSEN's Third Comet.— *R. A. S. Monthly Not.* xi, 1850–51, pp. 15, 16.

Report on Longitude, Telegraphic and Chronometric Operations.— *Coast Survey Report,* 1851, pp. 34, 35.

Elements of BRORSEN's Comet.—*Gould's Astr. Journ.* ii, 1851, p. 62

On the New Ring of *Saturn.*—*Am. Journ. Sci. and Arts,* Ser. 2, xii, 1851, pp. 133, 134.

Occultations of Stars Observed in 1846, 1847, 1848, 1849, 1850, continued from vol. iii of the Memoirs of the American Academy.—*Gould's Astr. Journ.* i, 1851, pp. 173–175.

Discovery of Comet by G. P. BOND, Aug. 29, 1850, pp. 213, 214; Observations on PETERSEN's Comet, 1850, pp. 215, 218; Observations on BRORSEN's Comet, 1850, pp. 221, 222.—*Astr. Nachr.* xxxi, 1851, pp. 213–222.

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Observations of the Comet of Aug. 29, 1850, etc.—*Gould's Astr. Journ.* i, pp. 141–142.

Positions of the Comet of August last, etc.—*Gould's Astr. Journ.* i, p. 155.

Moon Culminations, observed at H. C. O., etc.—*Gould's Astr. Journ.* i, p. 191.

Occultations of *Aldebaran*, *Jupiter* and *Regulus*, in 1848, 1849 and 1850.—*Gould's Astr. Journ.* i, 1851, p. 112.

Moon Culminations, observed at Cambridge, pp. 141–143; Double Stars, observed at Cambridge, 1848–49, pp. 144, 145.—*Am. Acad. Proc.* ii, 1852, pp. 141–145; communicated Feb. 6, 1849.

Observations on the Satellite of *Neptune*, 1847–48, pp. 136, 137; Observations on ENCKE's Comet, 1848, pp. 138, 139; Observations on the Eighth Satellite of *Saturn* (*Hyperion*), 1848–49, pp. 139, 140; Observations on PETERSEN's Second Comet, 1848, p. 140.—*Am. Acad. Proc.* ii, 1852, pp. 136–140; communicated Feb. 6, 1849.—*Astr. Nachr.* xxxi, 1851, pp. 35–42.

Moon Culminations, observed at Harvard Observatory, July, Aug., 1849.—*Gould's Astr. Journ.* i, 1851, p. 191; ii, 1852, p. 63.

Observations on MAUVAIS's Comet of July 4, 1847.—*Am. Acad. Proc.* ii, pp. 1, 2, 1852; continued from i, p. 169; communicated May 30, 1848.

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Observations of ENCKE's Comet, Jan., 1852.—*Gould's Astr. Journ.* ii, 1852, pp. 91–104.

Missing Star, Sept. 24, 1851.—*Gould's Astr. Journ.* ii, 1852, p. 104.

Observations on the First Comet of 1852.—*Gould's Astr. Journ.* ii, 1852, p. 174.

Observations upon ENCKE's Comet, made at the Observatory of Harvard College, Cambridge, U. S., with the great refractor.—*Astr. Nachr.* xxxiv, 1852, pp. 51–52, 225–228.

Observations on the Comet I of 1852, made at the Observatory of Harvard College, Cambridge, U. S. (May and June, 1852).—*Astr. Nachr.* xxxv, 1853, pp. 49, 50.

Report on Moon Culminations.—*Coast Survey Report*, 1853, appendix xxxii, p. 84.*

Records of Spring Governor.—*Ibid.* pp. 85–86.

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Second Comet of 1854.—*Gould's Astr. Journ.* iii, 1854, p. 189.

Extract from a Letter Giving Observations, etc., of VAN ARSDALE's Comet.—*R. A. S. Monthly Not.* xiv, 1854, p. 167; *Gould's Astr. Journ.* iii, 1854, pp. 159–160.

Moon Culminations.—*Coast Survey Report*, 1854, appendix xxxvii, p. 120.*

Chronometric Longitude Expedition.—*Coast Survey Report*, 1855, appendix xliii, pp. 275–276.

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History and Description of the Astronomical Observatory of Harvard College [with reports from 1846 to 1855].—*Annals* Harvard College Observatory, i, 1856, p. 1; *illus.*

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Observations on the Planet *Saturn.*—*Ibid.* ii, 1857, p. 1; *plates.*

Observations of *Isis* 42, Aug.–Dec., 1856.—*Gould's Astr. Journ.* v, p. 40.

Observations of *Polyhymnia*, by G. P. Bond.—*Gould's Astr. Journ.* v, p. 48.

Observations of *Isis* and *Polyhymnia*, made at Harvard College Observatory.—*Astr. Nachr.* xlvi, 1857, pp. 11, 12.

[Letter Describing Their Achievements in Stellar Photography.]—*R. A. S. Monthly Not.* xvii, 1857, pp. 230–232.

Moon Culminations, etc.—*Coast Survey Report*, 1857, appendix xxviii, p. 310–311.

Solar Parallax Deduced from Right Ascension Observations on *Mars* East and West of the Meridian, near the Opposition of 1849–50.—*Gould's Astr. Journ.* v, 1858, p. 53; letter dated April 28, 1857.

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Places of the Third Comet of 1858.—*Gould's Astr. Journ.* v, 1858, p. 134.

Fifth Comet of 1858 (Donati's).—*Am. Journ. Sci. and Arts*, Ser. 2, xxvi, 1858, pp. 433–434.

Observations for Coast Survey Longitude.—*Coast Survey Report*, 1858, p. 33; appendix xxii, p. 189.

Entdeckung Eines Cometen [Sept. 5, 1855, by H. P. Tuttle].—*Astr. Nachr.* xlix, 1859, pp. 141, 142.

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BOND, W. C. AND G. P., GRAHAM, J. D., [COMMUNICATED BY] BENJ. PEIRCE.

The Latitude of the Cambridge Observatory, in Massachusetts, determined from transits of stars over the prime vertical, observed during the months of December, 1844, and January, 1845.—*Am. Acad. Mem.*, New Ser. ii, 1846, pp. 183-203; given, also, in *Astr. Nachr.* xxv, 1847, pp. 97, 98.

LOVERING, JOSEPH, AND BOND, W. C.

An Account of the Magnetic Observations Made at the Observatory of Harvard University, Cambridge.—*Am. Acad. Mem.*, New Ser. ii, 1846, pp. 1–84; *plates*; also, *Sturgeon Ann. Electr.* viii, 1842, pp. 27–49, 89–112, 186–202.

BOND, W. C. AND G. P., AND OTHERS.

[Observations on the Eclipse of the Sun, Apr. 25, 1846.]—*Astr. Nachr.* xxiv, 1846, p. 196.

BOND, W. C. [AND G. P.]

Observations on the Comet of Mar. 4, 1847.—*Astr. Nachr.* xxv, 1847, pp. 355, 356.

BOND, W. C., W. C., JR., AND G. P.

Occultations and Eclipses Observed at Dorchester and Cambridge, Massachusetts. — *Am. Acad. Mem.*, New Ser., iii, 1848, pp. 67–74; communicated to the Acad., Aug. 12, 1846.

BOND, W. C. AND G. P.

Observations on the Belts and Satellites of *Jupiter*, and on certain nebulæ, Feb., 1848.—*Astr. Nachr.* xxx, 1850, pp. 93–96.

[The *Reports* of the U. S. Coast Survey for many years contain notes and reports from W. C. BOND, which have not been separately indexed here.—E. S. H.]

LIST OF THE SCIENTIFIC WRITINGS OF GEORGE PHILLIPS BOND

(COMPILED BY MRS. RICHARD F. BOND)

Discovery of a Comet, March 4, 1847, and observations.—*Am. Journ. Sci. and Arts*, iii, 1847, p. 443.

An Account of the Nebula in Andromeda.—*Am. Acad. Mem.*, New Ser., iii, 1848, pp. 75–86; *plate;* read before the Academy, March 7, 1848.

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On the Great Comet of 1844–45.—*Gould's Astr. Journ.* i, 1851, pp. 97–103.

Discovery and Observations of BOND's Comet, Aug. 29, 1850.—*R. A. S. Monthly Not.* xi, 1851, pp. 12–14, 63–110. [The first to bear his name, though the seventh or eighth of actual discovery.]

A Method of Finding the Distance of a Comet from the Earth.—*Gould's Astr. Journ.* i, 1851, pp. 115–116.

Letter Announcing His Discovery of a Telescopic Comet, Aug. 29, 1850, with position and elements.—*Gould's Astr. Journ.* i, 1851, pp. 118–141, 142, 155.

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Zone Catalogue of 6100 Stars, between 0° 40′ and 1° 0′ north declination, observed 1859-60.—*Annals* Harvard College Observatory (6), 1872.

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Report of Chronometric Expeditions of 1849–50–51. —*Coast Survey Report* 1854, appendix xlii, pp. 138*–142.*

Chronometric Expedition.—*Coast Survey Report*, appendix xxiii, pp. 182–191.

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Diagrams of the Planet *Saturn*, with explanations and results of observations, 1852, 1854, 1855.—*Am. Acad. Proc.* iii, 1857, pp. 186, 187; presented Feb. 13, 1855.

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[Results of an Examination of the Photographs of the Star *Mizar (ζ Ursae Majoris)* with its Companion, and the Neighboring Star *Alcor.*]—*Am. Acad. Proc.* iii, 1857, pp. 386–389; communicated May 12, 1857.

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Moon Culminations.—*Bruennow Astr. Not.* No. 14, 1860, pp. 105–108; No. 24, 1861, pp. 190–192.

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